

THE
AUSTRALASIAN JOURNAL
OF PHILOSOPHY

Volume XXXIV—1956

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KRAUS REPRINT LTD.

Nendeln, Liechtenstein

1966

Printed in Germany

Lessing-Druckerei – Wiesbaden

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OF PHILOSOPHY

VOL. 34

MAY, 1956

No. 1

THE REALITY OF THEORETICAL ENTITIES

By J. J. C. SMART

A theoretical physicist normally has a certain characteristic picture of the world. For him the world consists of unimaginably large numbers of electrons, protons, neutrons, photons, etc. These ultimate particles are for him the stuff of the world. We have found out about these particles by somewhat indirect methods, he would say, but nevertheless to assert that the world consists of such things differs only in degree from saying that a certain powder consists of small crystals, visible only under the microscope.¹ No one doubts that such crystals exist and are real in the same sense as that in which the powder exists and is real. Why not electrons too? On this view electrons are as real as chairs, and things consist of electrons, protons, etc., in the way that a wall consists of bricks.

Under the strain of certain conceptual difficulties arising out of relativity and quantum theory some physicists have adopted a quite different point of view. According to this doctrine the fundamental particles are no longer regarded as actual real things in the way that (perhaps) chairs, galvanometers, and crystals are, but are thought of rather as logical constructions out of our observations, fictions which enable us to state compendiously numerous apparently heterogeneous laws of the interrelations of observations. In this they concur with older philosophical scientists who advocated a similar doctrine under the strain of certain sceptical arguments of a general philosophical nature, hinging on concepts such as *explanation*, *necessity* and *real*. Often the two sorts of conceptual difficulty are intermingled, and frequently the arguments of a scientist who advocates that electrons are only logical constructions out of observations hinge much more on general concepts like 'real', 'explanation', and 'necessity' than on specific unclarities about electrons, simultaneity, or indeterminacy. Frequently the view that electrons are logical constructions out of observations with

¹ Cf. Max Born, 'Physical Reality', *Philosophical Quarterly*, vol. 3 (1953), pp. 139-149, especially p. 141.

cathode ray tubes, galvanometers, and so on, is combined with and draws support from a phenomenalist view that the cathode ray tubes and galvanometers themselves are only logical constructions out of sense-data.¹ On this view theoretical entities are constructions out of constructions, and so do not differ importantly from our familiar tables and chairs, which are themselves only constructions. On this extreme phenomenalist form of the 'logical construction' doctrine theoretical entities are once more assimilated to everyday objects, but at the price of denying bedrock reality to both.

I should make it clear that in this exposition I am not using the term 'logical construction' in its strictest sense. In discussing the view that theoretical entities are logical constructions out of observations with cathode ray tubes, galvanometers, etc., I am not discussing the view that statements about theoretical entities can be translated into statements about observations, for this view has been conclusively refuted by F. P. Ramsey and R. B. Braithwaite.² I am considering the allied view that somehow or other talk about electrons is just a matter of talking abstractly about observations, in a way similar to, though different in some respects from, the way that talk about nations is a matter of talk about people. (Even though nation statements cannot be translated into person statements.)

In this dispute most professional philosophers seem at present to be more or less on the side of the 'logical construction' doctrine (though not, usually, in its extreme phenomenalist form). Thus Mayo says 'If theoretical concepts are only to be explained by referring to the particular theory in which they have a place, and if the only justification for accepting a particular theory as established is that it enables us to explain known facts and successfully predict new ones, then it follows that we gain nothing by either attaching or withholding the epithet "real".' To say that electrons are real can be to say no more than that a certain concept occurs in an established theory. This is to give electrons a very thin sort of reality: it puts them on a level with lines of force, accelerations, and even geometrical points, all of which are concepts which occur within physical theory. Can we do no more than this? Even if the physicist who says that electrons are as real as microscopic crystals of a powder should be wrong, is he not making a real distinction? Such a man would by no means have an inclination to say that

¹ Cf. H. Margenau, *The Nature of Physical Reality*, 1950.

² Ramsey, 'Theories', in *The Foundations of Mathematics*; Braithwaite, *Scientific Explanation*, chapter 3.

³ B. Mayo, 'The Existence of Theoretical Entities', *Penguin Science News*, 32, p. 17 (1954).

a line of force, still less a geometrical point, is on a par with a small crystal of the powder. Toulmin, in what is perhaps the best and clearest short discussion of the problem of the reality of theoretical entities,¹ after adopting a position very like that taken up in Mayo's later article, does do something to restore the distinction in question. 'Do atoms, genes, electrons, fields, etc. exist?' has, he suggests, the force of 'is there anything to show for them?' 'To a working physicist, the question "Do neutrinos exist?" acts as an invitation to "produce a neutrino", preferably by making it *visible*.'² Obviously, however, one cannot literally *see* an electron or a neutrino, and so 'our problem is accordingly complicated by the need to decide what is to count as "producing" a neutrino, a field or a gene. It is not obvious what sorts of things ought to count: certain things are, however, generally regarded as acceptable—for instance, cloud-chamber pictures of X-ray tracks, electron microscope photographs or, as a second-best, audible clicks from a Geiger counter.' The trouble about this is the apparent arbitrariness of the collection of procedures which count as 'producing' an entity or 'something to show' for it. One has an inclination to say that lines of force are not entities in the way that electrons are, but there can plausibly be said to be 'something to show' for them: indeed magnetic lines of force can be in a sense shown very vividly by scattering iron filings near the poles of a magnet. And on the other hand even so vivid a thing as seeing a large molecule in the electron microscope can be thought of sceptically. What is seen, it can be said, are only dark patches on a cathode ray screen, meaningless without the theory of the electron microscope which enables us to interpret them, and the large molecule is just a fiction which may enable us to conflate these observations with others of a different sort.

To put my cards on the table, let me say that there is a sense in which electrons, neutrinos, photons, and so on are real things (whose existence is no more abstract than that of the small crystals of our powder) as opposed to lines of force, points of space, or components of momentum, which can plausibly be dismissed as constructions or abstractions. (Like the 'factors' of factor analysis in psychology, which no reputable psychologist thinks of as actual things.) Of course, on some theoretical pictures, an electron might be described as a singularity in a field of force, but then this field, whether Descartes' extension or the modern space-time, is thought of intuitively as an extensive

¹ *The Philosophy of Science*, 1953, pp. 134-139.

² *Op. cit.*, p. 136.

substance, whose warpings constitute the field. This field picture is just one way of describing what is described by the discrete atomistic picture, and which picture is adopted does not affect my argument. Let us see how far my crude statement of a naïvely realistic picture can be discredited by philosophical analysis. One can already hear one's philosophical friends saying impatiently: 'Of course this is a typical philosophical puzzle. When in philosophy we ask whether we should say "yes" or "no" the answer is always "yes and no" or "neither and both". Of course there are features of the logic of "electron" that pull us in the direction of classifying electrons with bricks. And there are other features that pull us in the direction of classifying them with factors or geometrical points. When all these features have been described, then say what you like.' But is the answer to the question necessarily 'yes and no'? Might it not be at any rate very much more 'yes' than 'no' or very much more 'no' than 'yes'?

An initial difficulty that presents itself is this: that it is not easy to think of situations in which the question 'do electrons exist?' would naturally arise. A question like 'do unicorns exist?' is a clear question because the concept of a unicorn is a common-sense one which can be easily enough explained in terms of other concepts: a unicorn is a quadruped with one central horn. Whether this concept applies to anything or not is something we can find out in a straightforward empirical way, say by searching the southern Sahara desert. In order to learn the use of the word 'electron', however, we have to learn what an electric charge is, what a cathode and an anode are, about J. J. Thomson's experiments, Millikan's experiments, and so on. Fully to understand the word as it is now used we must furthermore understand the part 'electron' plays in theory, know something of the structure of the atom, and about the successively more refined developments of quantum mechanics that have grown up. Only after all this will we know how to use the word 'electron' as physicists use it. Learning the meaning of the word is something which comes to something little short of learning physics. When we have learnt a modicum of physics the question 'do electrons exist?' no longer arises. Before we reach this stage this question has no very clear meaning. Nevertheless at an early stage it has *some* meaning. We can imagine Thomson and Millikan summing up the results of their experiments by saying 'electrons exist': i.e. 'a smallest discrete electric charge exists, which appears to be associated with a particle'. Even now, though the question 'do electrons exist?' does not arise, that is not, I think, to say

that it has no meaning.¹ The question 'does anything exist?' does not arise, but surely it has meaning and the answer to it is 'yes'. We can imagine present-day physical concepts being overthrown by new ideas, and its being denied that there are any quanta of electric charge. (We can *imagine* this: I do not say that we can believe this.) In such a case 'do electrons exist?' could be answered by 'no'. If a future historian of science made sufficiently arduous researches he could come to learn the rules according to which the word 'electron' was once used: the fact that on our supposition the theory of electrons and these rules with it would now be on the scrap heap does not affect the matter. And if asked 'do electrons exist?' he could answer 'no'. But admittedly the question 'do electrons exist?' would not then naturally arise. (Nor does it arise now when the theory is taken for granted.)

Since the question 'do electrons exist?' does not naturally arise it follows that if anyone did ask it he would be taken most naturally to be asking a question about the logical status of the concept 'electron'. There are two ways in which a question of this sort might be asked. In the first place we could consider the question as a quasi-empirical one. That is, we might ask whether the experimental facts are such that they push us in the direction of thinking of electrons as actual particles rather than as avowed fictions like lines of force or geometrical points. In the second place, when this quasi-empirical question has been answered in the affirmative there remains a purely logical question: what is it to think of electrons as actual things, not as avowed fictions? Are not these actual particles themselves in a subtle way perhaps no more than theoretical constructions? That is, the question 'are electrons real?' can be construed in a wholly non-empirical way as a question not about electrons but about the use of the word 'electron'. In order to substantiate this last suggestion and in order to see just what it amounts to, it will be necessary to pay some attention to the logic of the word 'real'.

What do we mean when we say that elephants are real? At first sight this is to say the same as that elephants exist, or that there are elephants. Nevertheless if we look closer we can see that '. . . is real' normally does a somewhat different job from 'exists' or 'there is a . . .'. There is no harm in saying that there is a prime number between 15 and 20 and that such a prime number exists, but to say that a prime number between 15 and 20 is real would be very odd. There is no harm in

¹ In contradiction to what I have argued elsewhere. 'The existence of God', *Church Quarterly Review*, April, 1955.

saying that there is a mad king in one of Shakespeare's plays, but most of us would deny that such a person was real. Closely connected with this (grammatically) predicative use of 'real', in which we rebut the suggestion that the thing in question is an abstraction, or a fiction, or imaginary, there is a purely adjectival use in which we say that something is 'a real such-and-such'. When we say that a diamond is a real one we rebut the suggestion that it is an imitation one.¹ To say that the conquest of Everest was a real achievement is to rebut the suggestion that it was a meretricious one. Notice that if we doubt whether a diamond is a real one or whether a piece of material is real silk or whether an achievement is a real achievement we do not doubt whether the thing in question exists (or whether the event in question actually happened). Furthermore, and this is more important, there is no category difference between the real thing and the imitation, counterfeit, or meretricious one. The real achievement and the meretricious one are both events, the real silk and the artificial silk are both stuffs, and the real diamond and the imitation diamond are both things. On the other hand, when we use the word 'real' in the predicative way, as in 'fairies are not real: they are imaginary', we are making a category distinction. A real man and an imaginary man are not two examples of the same sort of thing in the way that a real diamond and an imitation one are. 'Fairies are not real' is a disguised statement about the way the word 'fairy' normally works: that is, only very naïve people play the same language game with the word 'fairies' as they do with 'lions' and 'men'. There are thus two logically different ways in which we use 'real'. We use it to make a distinction of fact (real versus imitation, etc.) and we use it to make one of a number of category distinctions (real versus imaginary, fictional, abstract, etc.). (There is not just *one* category distinction that is made: for example the distinction real (actual) versus imaginary is quite different from that of real (concrete) versus abstract.) I have tried to relate these two sorts of uses of 'real' to differences in grammatical usage: the predicative, '... is real', and the purely adjectival, '... is a real ...'. However, grammar is not an infallible guide. We often say 'fairies are not real things' or 'fairies are not real creatures' instead of 'fairies are not real', and we can say 'that diamond is not real', meaning 'that stone is not a real diamond'. Note, however, that in the former case we tend to finish the sentence with a category, or near-category, word like 'thing' or 'creature'. To use a non-category word like 'men' and say 'fairies are not

¹ Cf. Mayo, *op. cit.*

real men' would suggest that they were in some way artificial or imitation men, which is not what we want to say at all.

Mayo (*op. cit.*) says: 'Anyone who wishes to maintain that electrons exist or are real must be prepared to ask himself what possible sorts of defectiveness or lack of qualification he is concerned to deny.' We may question the implied suggestion that to say 'electrons exist' is much the same as to say 'electrons are real', and so let us for clarity replace 'exist or are real' in the above statement simply by 'are real'. Then, suggests Mayo, if we say that electrons are real we are denying some sort of lack of qualification or defectiveness. If we cannot say what sort of lack of qualification or defectiveness is in question then our assertion lacks a clear meaning. On the other hand we have seen that though 'electrons are real' is closely connected with 'electrons are real such-and-such's' there is a difference between the two statements. If a man said 'electrons are not real particles' we might understand a certain sort of defectiveness to be in his mind: say, that electrons are wave packets rather than particles in the traditional sense. But if a man said 'electrons are not real' he could be described *either* as not asserting a defectiveness at all *or* as asserting a logically queer kind of defectiveness. To take a parallel example. If a man said that numbers are not real he would perhaps mean that numbers are not concrete things but abstractions. Some philosophers like Nelson Goodman might object to numbers on this score: a universe containing such dubious entities could in a sense be described as a universe containing defective elements. Goodman could in a sense be described as a man who regarded abstractions as defective, not fit for his world as concrete objects are. On the other hand we can also see that the above way of putting the matter is highly misleading. If there are (in a sense) no such things as abstractions how can we say that there are such things but that they are defective? It is clear that what Goodman would regard as defective is not the abstractions themselves but certain words (words which purport to name such entities). What Goodman wants is a certain sort of language, a nominalistic one. It is not numbers that are defective in his view: it is numerals (at least as used in a certain way).

In the same sort of way, if we say that electrons are real or not real we are not (to use Mayo's words again) denying or asserting some defectiveness or lack of qualification (as we would be if we said that they were or were not real particles), or, to put it differently, we would be denying or asserting defectiveness or lack of qualification in a queer sort of way.

The sort of defectiveness or lack of qualification we would have in mind would be 'being an abstraction' or 'being a theoretical fiction' or 'being a logical construction', as opposed to being a concrete entity like a chair, a microscopic crystal, or a microbe. If we spoke in this way we should be speaking inaccurately or misleadingly: it would be better to say that what we are really accusing of defectiveness is the word 'electron', that is, we should be saying that the word 'electron' belonged to a certain logical category. And if we have a preference for a language containing certain logical categories rather than others, or if we prefer to talk about concrete entities rather than abstractions, to say 'electrons are not real' would be to complain that though the naive scientist supposes that 'electron' behaves like 'microbe' it really behaves more like, say, 'root mean square velocity'. That is, our scientific language is accused of being less concrete than we had hoped.

Let us go back to considering the fine powder whose crystals are visible only under the microscope. We feel confident enough (in a way we do not feel confident about electrons) that the crystals are concrete entities just as bricks and houses are. What is the basis of our confidence?

Suppose a person looks through a very low power microscope. He sees a lump of sugar, say, looking twice as large. He does not need to know the theory of the microscope to know that what he sees is a lump of sugar. It looks just like a lump of sugar, only bigger. Even a savage, who knew nothing of optics, would tend to say that what he saw was a lump of sugar. It would be even more likely that he would say that what he saw through the lenses was a lump of sugar if he also had a look with the naked eye at the lump of sugar on the tray of the microscope. Compare the way children automatically say 'car' when they see a picture of a car: no doubt they do this even before they have any conception of what a picture is. (Certainly they may before they learn the use of the word 'picture'.)

In this way we get to think that a microscope shows us what things would look like to us if they were many times bigger (or we many times smaller). We thus think of the minute crystals of the powder as only contingently invisible to our naked vision, and we think of it as only contingent that they do not seem as substantial to our naked vision as bricks and motor cars now do. This way of thought is naturally strengthened by what we know of optics.

When we pass from what we see with ordinary microscopes to what we see with the ultra-microscope or the electron micro-

scope a different situation presents itself. In such a case we could not expect our savage to interpret what he sees: here knowledge of optics or electron optics is essential. Nevertheless, given this knowledge, we can see that it is still a contingent fact that we cannot see with our naked eyes what we see with the microscope. For example, if there were as many electron beams through space as there now are photon beams, and if we had ultra-microscopic eyes somehow constructed on the electron microscope principle, we might be as much at home in the world of individual living cells as we now are in the world of dogs and cows. (I neglect here certain difficulties: one is that if there were as many electrons flying around as there are photons now it might be incompatible with what we know of biophysics that our living cells should have the structure they now have.)

It now looks no more than a contingent fact that we cannot see even such *recherché* entities as bacteria, viruses, cells, and even large molecules. What if we go further and say that it is just a contingent fact that we cannot see electrons, protons, mesons, photons, etc.? Here, however, theoretical considerations come in. In any ordinary sense of 'see' what we see must have a definite position and shape, and our seeing of it must leave it substantially unaffected. These conditions could not hold with fundamental particles. Since 'electron' etc. get their meaning from the part they play in theory, we cannot say that it is only a contingent fact that those conditions hold which make it physically impossible for us to see them. In the case of a photon it is particularly obvious that however microscopic we were we could never see a photon. Photons explain seeing: they therefore cannot themselves be seen. For would one see a photon by means of a further photon? Furthermore, if photons could be seen we could never see anything else: they would get in the way of whatever else we wanted to see.¹ The whole conception of 'seeing a photon' is self-contradictory. 'Photon' gets its meaning from the part the word plays in prevailing theory, and this theory in turn rules out the possibility of photons being seen by anything, however microscopic.

There thus seem to be two reasons, and two only, for putting the names of the fundamental particles in a different category from 'brick', 'microscopic crystal', and 'cell'. One is that there are theoretical reasons why they could never be perceived: it is therefore not a contingent fact that we do not talk about them in a language at all analogous to our ordinary everyday language of perceptible objects. The second is that if the theories in

¹ I think that I once heard this point made by Dingle.

which their names play a part are given up or radically modified (as they may well be) we shall give up even talking of electrons, protons, etc. This, however, is not as solid an objection as seems at first sight. There is no reason to suppose that 'electron' will ever suffer the fate of 'phlogiston'. Whatever it is that we now describe (possibly misdescribe) in our present theoretical language will still have to be described (or misdescribed) in the new language. After all, when in the 1920's people stopped talking about particles in the sense of the Bohr atom and started talking about wave packets, 'wave packet' still tried to describe what 'particle' did before. The situation is not all that different from the sea serpents which later mariners describe as schools of porpoises: there is something described or misdescribed by both the old and the modern sailors.

It might be objected here that the new description might involve a category difference. But this might be so even with the sea-serpent: 'sea-serpent' might misdescribe not a group of porpoises but a wave, and a wave is of a different category from a serpent. Nevertheless the mariners who said they saw a serpent and those who said they saw a wave might, in a sense, be said to be 'describing the same thing'. Category distinctions are in any case by no means hard and fast: if you try hard enough you can exhibit any word as in a different category from any other word.¹

The situation seems to be this: there are things which we cannot describe in detail but about which we can already say quite a lot. We cannot see them and there are theoretical reasons why we could never in principle see them. And that is about all there is to it.

I should like, furthermore, to defend the no doubt naïve view of most physicists that modern theoretical science gives us a truer picture of the world than does the language of ordinary common sense. This is not to say, of course, that the propositions of everyday life are not true. Of course they are true, and I thoroughly agree with philosophers who oppose any suggestion that there is an incompatibility between what common sense says and what science tells us. (For example, Ryle, in *Dilemmas*, chapters V and VI.) That is, I want to specify a sense in which physical theory gives us 'a truer picture of' the world than does common sense, which does not entail that theoretical statements are more frequently true than common-sense ones. (Indeed it is somewhat unnatural to use the terminology of 'true' and 'false'

¹ See my 'Note on Categories', *British Journal for the Philosophy of Science*, vol. 4, pp. 227-228.

in connection with theoretical statements at all.) Now there is one very clear sense in which we can say that physical theory gives us a truer picture of the world than common sense does. This is that many of the concepts of common life are anthropocentric. It is colour concepts that I shall single out for consideration, but there are of course others. (For example 'nauseating', 'sickly', 'dazzling', 'sweet'.) It is a well known fact that various mixtures of wavelengths of light will occasion the same discriminatory response from a normal person as pure light of a certain single wavelength will. Just what mixtures of light are equivalent to one another in this respect is a rather arbitrary matter, depending on the peculiarly complicated structure of the eye and nervous system. We have by no means got 'spectroscopic eyes'.¹ It would be much easier to construct an instrument which would distinguish between any two different distributions of intensity of light among wavelengths than to construct an instrument which makes the particular discriminations, and lack of discriminations, that the human eye makes. (I have no doubt that the latter task is in practice impossible.) This shows the arbitrary and anthropocentric nature of our colour concepts. There is no reason why Martians should have the colour concepts we have. We would, however, expect them to have the same sort of physics, though it might be more or less advanced than ours. 'A truer picture', then, means 'less (or not at all) anthropocentric'. One way in which scientific language is less anthropocentric than ordinary language is that our descriptions of the world are not confined to macroscopic objects. And with this goes greater detail of description and proportionately great simplicity of explanation.

I have been trying to cut down to life size the philosophical objections so commonly brought against regarding theoretical entities as concrete things. It is true that many physicists have tended to adopt a sceptical attitude to the concrete reality of theoretical entities under the stress of the difficulty of reconciling a more or less realistic view with the present state of atomic physics. It is beyond my province to deal with such objections: I should merely like to state that I have on my side one very eminent physicist (Max Born) whose arguments² against such objections seem to me eminently sensible. It is also evident that most physicists who base their arguments on the modern state of physics often give expression to phenomenalist strains of thought, and it is open to question how many of their difficulties

¹ Cf. Pope, *Essay on Man*: 'Why has not man a microscopic eye? For this plain reason: man is not a fly.'

² Max Born, *op. cit.*

stem from quandaries about recondite physical concepts, and how many, after all, are just the familiar philosophical parrot cries.

The naïve physicist thinks that his science forces us to see the world differently and more truly. I have tried to defend him in this view. The modern tendency in philosophy is to be opposed to phenomenism about tables and chairs but to be phenomenist about electrons and protons. Philosophers thus tend to give an ontological priority to everyday concepts and they come to have 'a sharply . . . definite view of the world: a world of solid and manageable objects, without hidden recesses, each visibly functioning in its own appropriate pattern'.¹ They are wedded to the concepts of common sense every bit as much as the mediæval pre-Galilean philosophers were, but they are less open about it.

Adelaide University.

¹The quotation is from the last paragraph of S. Hampshire's critical notice of Ryle's *Concept of Mind* (*Mind*, vol. 59, 1950). I do not think, however, that a phenomenist doctrine about theoretical entities is in fact entailed by Ryle's book.

CATEGORY MISTAKES¹

By A. J. BAKER

In talking about "category mistakes" I want to consider the view, developed especially by Professor Ryle, that there are various differences of category or type within ordinary language. I shall not be concerned with theories of the categories as advanced by philosophers like Kant, Hegel and Alexander, nor with the theory of types as used by Russell to deal with paradoxes, mainly in mathematical logic, though there are connections between these views and the distinction of categories in ordinary language. But Russell does provide a starting point in his extension of the theory of types to deal with ordinary language.² In doing this, however, he talked about types of entity and types of fact and this way of talking itself led to a paradox, as was pointed out by Max Black.³ Without discussing this, if we adapt what Russell said to differences of type amongst *expressions*, his view was that two expressions belong to the same type if they can fill the same sentence-frame. For instance, given "So and so is a philosopher" as a sentence-frame, we can say both that "Socrates is a philosopher" and "Plato is a philosopher" and negatively that "Caligula is not a philosopher", so that, according to Russell, "Socrates", "Plato" and "Caligula" are all of the same type. But, as Ryle has shown,⁴ this account is too simple. Occurrence of expressions in the same sentence-frame is not enough to ensure sameness of type or category, for expressions which are similar in regard to one sentence-frame may be dissimilar in regard to a different sentence-frame. So what we have is really a negative criterion: given two expressions, if there is at least one sentence-frame in which one expression can occur and the other cannot, the two expressions belong to different categories.

Now the way in which these differences of category are commonly noticed is by observation of category mistakes. For example, to say "Time began a million years ago" is to make a category mistake, and since, in contrast with this, we can, for

¹ Based on a paper read at the Annual Congress of the A.A.P.P. in August, 1955.

² Cf. "Logical Atomism" in *Contemporary British Philosophy*, Vol. I.

³ In "Russell's Philosophy of Language" in *The Philosophy of Bertrand Russell*.

⁴ Cf. "Categories", *Proceedings Aristotelian Society*, 1937-38.

instance, say without absurdity, "Agriculture began a million years ago", it follows that "time" and "agriculture" belong to different categories. Or, in general, two expressions belong to different categories if there is a sentence-frame in which the occurrence of one expression does, and of the other does not, generate a category mistake.

In these preliminary remarks I have spoken of differences of category amongst expressions, but this needs to be qualified. Ryle's preferred formulation is, I think, that category differences are differences amongst the different logical powers of *sentences*; their logical powers being the different logical relations they may have to other sentences. It follows that differences of category amongst words and phrases are derived from differences amongst the sentences in which they can occur. But, allowing this, I want, for convenience, to go on talking in a shorthand way of category differences amongst words and phrases.

I should also say that I have no doubt that the implied method of doing philosophy by detecting category differences is a fruitful one, especially since in best philosophical practice it is used not so much as a way of cataloguing category differences, but as a working method for isolating problems to be tackled. But while the method in action may be the important thing rather than commentaries on it, I want to raise questions about its status. In particular, there is the general question: by what criteria are category differences established, or, what comes to the same thing, what is the test for a category mistake; and, again, can category mistakes on their own bear the weight that is sometimes placed on them; in particular, is the discovery, by itself, of a category mistake as philosophically illuminating as is assumed by some contemporary writers?

One difficulty with the category mistake method is that it appears to go too far, to generate too many categories—not only from metaphorical and extended uses of expressions but also from their literal uses. This is a point made by Professor Smart in "A Note on Categories",¹ where, to mention one of his examples, he argues that the sentence-frame method leads to the conclusion that "chair" and "bed" belong to different categories, for we can say "The seat of the chair is hard" but not "The seat of the bed is hard"; and he goes on to say "If furniture words do not form a category, we may well ask what do". In addition to this there are recent accounts of synonyms according to which for any two alleged synonyms we can find a difference of use:

¹ In *The British Journal for the Philosophy of Science*, November, 1953.

as an extreme example, "eye-doctor" will go into the sentence-frame "An oculist is a such and such", but "oculist" won't; we can't say (literally) "An oculist is an oculist". So if differences like these count as category differences, the position will be that every word has its own category, every sentence its own logic, and, further, in view of systematic ambiguity, *i.e.*, of the fact that the same word may have a systematic series of different uses, it could easily happen that any word has many categories. And this is a conclusion which, apart from anything else, would deprive the statement that two expressions differ in category of a point.

The question, as a result, is whether there is any way of breaking down the wholesale generation of category differences. One partial answer which has been given is that to say that two expressions differ in category should not be taken to mean that they belong just to two exclusive categories. If we bring together the positive and negative criteria I mentioned earlier two expressions may be shown by one sentence-frame to have a category in common and by another sentence-frame also to belong to different categories. This denial of exclusive categories is especially useful in the case of more general examples of logic where we frequently want to say that two expressions have logical similarities and also logical dissimilarities; for instance, when we compare definite descriptions with proper names or "because" sentences with "if, then" sentences. Accordingly, in the case of the chair and the bed we could say that while they are dissimilar in some ways they also belong to a common category and for this reason can be classed together. However, on this view there will still be an enormous number of categories: while it will now be possible to group expressions together in some respects, if every expression has special uses of its own there will still be at least as many categories as there are expressions.

This seems to me to be still a multiplication of categories beyond necessity. But is it a conclusion forced on us? The answer depends on what exactly we take to be a category mistake or the source of a category difference. It may be said that category mistakes reveal themselves at once; that when we are confronted by an offending statement we simply recognize that it is absurd or inappropriate. But there are many claimants for the title "absurd". Statements like "The earth is flat", "Someone will run the four-minute mile", "Orwell's picture of 1984 will be realized", all are or have been regarded as absurd by some people simply because of the state of their beliefs or the strength

of their wishes. So the question is whether an account can be given of philosophically interesting absurdity. An account which does attempt to do this is that offered by Ryle in *Philosophical Arguments*¹ (though I am not sure whether it is his main view). According to this a statement which is absurd or makes a category mistake can be shown to generate contradictions or antinomies. For example, "Time began a million years ago" can be shown to involve a category mistake because, quoting Ryle, "It tries to say what cannot be significantly said, viz. that there was a moment before which there was no possibility of anything being before anything else, which contains a patent contradiction".

I want to contrast examples of this sort with certain other types of statement which may be said to be absurd. First there are statements like those above which are absurd or not absurd, depending on the speaker's attitude. Second, there are statements whose absurdity is purely linguistic, as in saying "I hit a blow" instead of "I strike a blow", or "The fox has a tail" instead of "The fox has a brush", or in speaking of "a flock of lions" instead of "a pride of lions". It does not appear that philosophical contradictions arise here. If someone persists in speaking of "a flock of lions" he may be misusing language, but he is not being a bad philosopher.

Third, and more important, there are statements of the same kind as Carnap's "This stone is thinking of Vienna", such as "That baby is embarrassed", "Gum-trees have a poor appreciation of poetry", "Worms are virtuous". It is sometimes suggested that statements of this kind make category mistakes on the ground that (for instance) "virtuous" and "not virtuous" have no use applied to worms, or that "embarrassed" and "not embarrassed" have no use applied to babies. But while there is something inappropriate about these statements it doesn't follow that they are logically inappropriate. It seems to me that they are simply false, even though, like denials of Locke's trifling propositions, they are trivial falsehoods. The opposed view, that they involve category mistakes, appears to rest on certain dubious assumptions. In particular, it appears to assume that the account that used to be given of "universes of discourse" or "limited universes" was correct on at least one point, that we can know at once, and indubitably, to which universes of discourse or fields of reference different expressions belong. At the same time the assumption appears to be made that the distinctions amongst familiar genera and species establish themselves. Given these assumptions it follows that we can know with certainty at least

¹O.U.P., 1945.

what questions cannot arise within a given universe of discourse or a given genus. We all know, the argument runs, that only, say, in the limited universe, men, do the predicates "appreciate poetry" and "fail to appreciate poetry" arise; hence, to misapply them, *i.e.*, to apply them outside their universe, is to make a category mistake. (On the other hand, statements which are regarded not as absurd but as merely obviously false will have to be treated differently; for instance, if "The earth is flat" is taken to be just empirically false, the reason will be that the predicates "flat" and "not flat" can properly be applied to physical objects including the earth.)

But suppose we chose to insist on knowing why a predicate cannot be applied outside its universe or field (or how, precisely, to distinguish absurd from merely false applications). It seems clear that an explanation would have to be given in terms of obvious matters of fact. Thus, in the case of "Gum-trees have a poor appreciation of poetry", the explanation would be something like this: Gum-trees can neither appreciate nor fail to appreciate poetry because all things which either appreciate or fail to appreciate poetry are men, and gum-trees are not men. But if the original statement is explained in this way as a category mistake the question arises: what is the status of the explanatory statements? Two answers may be given, that they are empirically true or, alternatively, that to deny them is also to make category mistakes. On the first alternative it follows that a general statement like "All things which appreciate or fail to appreciate poetry are men" can be empirically denied, whereas a less general statement like "Gum-trees can neither appreciate nor fail to appreciate poetry" is not open to empirical assertion or denial. But this involves an arbitrary decision to make a logical contrast between grades of generality, for no special contradiction has been shown to arise from denying the second statement. Of course, it may be said that if anyone knows the general statement he cannot, without contradiction, deny the less general statement. But this depends on a general fact about logic. If p and q entail r and someone asserts p , q and not- r , he might be said to make a category mistake, but it is a category mistake about entailment in general, not about the empirically false statement not- r . (Otherwise, if not- r were just for this reason a category mistake, every false statement could be a category mistake.)

The second alternative is to maintain that to deny the general statement is also to make a category mistake, *i.e.*, that anyone who understands the statement necessarily knows it to be true. But in this case there is still the question how to know where

to draw the line between statements of this sort and other very general statements which are taken merely to be obviously true. An answer may be given by appealing to the standard or established use of expressions, but, whatever use may be made of this appeal in other connections, to make it in the case of the examples I am talking about is to adopt a clearly relative position. For instance, it would not be surprising to hear of primitive societies in which it was believed that trees have human characteristics, and it is still possible to know how to use the word "tree" (*i.e.*, without knowing all about it, all the possible rules for its use) without knowing that trees do not have various human features. Similarly, it is at least logically possible to be mistaken about the sophistication of babies or the ethics of worms. So appeal to standard use, made consistently in all cases of this sort, would have to be appeal to standard beliefs, *i.e.*, to what the majority, or at least the educated majority, believe. And if this is what decides when category mistakes occur, it follows that categories may conceivably differ at different times and in different societies, and that the emergence of new category differences may coincide with the general acceptance of previously disputable statements as indisputably false. But I should argue that this sort of appeal to the majority is an odd criterion of truth, even of philosophical truth—or, put differently, I take it that the concept of category differences, if it is to be of philosophical interest, is concerned not with relative, but with fixed and recurring distinctions.

I find the same difficulties with another test of category differences, which has been used by Ryle on occasions, that of asking whether expressions can be coupled by "and" or "or". This test often does point to distinctions of logical interest, but if it is applied without qualification it will follow that pairs of expressions like "good-tempered and inorganic", "not hungry or heavier than air", involve category mistakes. But to say this would be to rely just on standard beliefs. We don't, in fact, couple expressions like "good-tempered" and "inorganic" because we find we have no use for them, but this is simply because we believe as a matter of fact that nothing inorganic is good-tempered or has a temper at all. Moreover, many of our ordinary couplings by "and" and "or" are very restricted indeed. If someone were to say "He was lonely and interested in arithmetic" or "The magazine was illustrated and weighed three ounces", the statements would be felt to be inappropriate, but this is because they conflict with what we conventionally expect and not because the coupled expressions cannot describe the same things.

Professor Smart's example of the seat of the chair and the bed is more complicated. If we just had the statement "The bed has a seat" it would be agreed, I suppose, that (as with "The earth is flat") we have simply a false statement. So it is the combination of "seat" and "bed" as in "The seat of the bed is hard" which generates the category mistake. The source of this mistake is still the obvious truth that beds do not have seats, but there is the complication that here a property is ascribed to what does not exist. We have, that is, a case in which, in Mr. Strawson's language, use of a phrase presupposes that the phrase refers to something, but, since it does not, the statement in which the phrase occurs is a spurious statement. But if this is enough to show that "chair" and "bed" are of different categories, the same could be done with any two object-expressions whatever (unless, indeed, there are concrete falsifications of the identity of indiscernibles); for, given two object-expressions "A" and "B", if we find a property C which belongs to B and not to A, we can then go on to make statements of the forms "AC is D" and "BC is D", such that the first, but not the second, is a spurious statement. But I think it would be wrong to say that every spurious statement of this sort qualifies as a special category mistake—it would be like saying that every separate syllogism with undistributed middle commits a special fallacy. I should prefer to say that the mistake of logical interest involved is simply the general one of purporting to say something true or false and in fact failing to do so.

I want now to consider some examples which may with more justice be said to involve category mistakes, *i.e.*, examples which do not just depend on empirical falsity but lead to antinomies or contradictions. First, there are various examples that could be found in formal logic, though I shall say very little about these. If we apply the sentence-frame test in these cases, grammar alone is usually sufficient to show that the results are absurd. We can't, for instance, say "Therefore (But, If etc.) went to town yesterday". And this absurdity does not arise merely because of what we happen to believe, nor is it an accident of grammar like the fact that we cannot say "He was the taller of the three"; it is, of course, connected with the fact that logical expressions of this sort cannot, without philosophical contradiction, be treated as referring expressions.

But, considering what Ryle calls "informal logic", examples of statements which it is easy to see can lead not merely to empirical but to philosophical errors are those dealing with abstractions and fictions. Even here, however, there can be

dispute about whether these statements exhibit category mistakes. Thus Mr. Passmore¹ has criticized Ryle's account of why the statement "Mr. Pickwick is a fictitious person" is logically misleading. According to Ryle, the statement, if taken literally, leads to contradictions; it leads, for example, to the statement that "Mr. Pickwick was born in such and such a year". Passmore, however, points out (to abbreviate his argument) that the contradiction arises only if we accept certain unstated propositions, such as the propositions "All persons are born in a certain year", "No characters from fiction are born in a certain year". Given these, it is contradictory to go on to say that a character from fiction, like Mr. Pickwick, is a person; but the contradiction is avoided by anyone who refuses to accept either of the unstated propositions. But, while Passmore is quite correct in drawing attention to these unstated propositions, it must be allowed that there is a difference between this example and the ones I discussed earlier. In the present case, to deny the unstated propositions would be to face special absurdities. Thus, anyone who seriously maintained that some persons do not have a date of birth or that characters from fiction do have a date of birth would presumably be led to say that there are persons or entities of a very special kind and would then find it hard to justify his ontology; by classical procedure he might be led to say that his special entities subsist and then be unable to give an account of their similarities to and differences from ordinary things or persons. In other words, it is not so much that the original statement is, as it stands, absurd, but that a person asserting it could be forced into a philosophically defenceless position.

The same point can be made about another familiar illustration, that of abstract words. Statements like "Equality is easily annoyed", "Greenness is hexagonal" do involve category mistakes, but this is because they can be shown to lead to contradictions rather than because they can be read off at once as absurd. For if the test is merely that they sound odd, or that no one would want to say them, no distinction has been offered between these statements and obvious empirical falsehoods. (Indeed, the view might be presented that they are merely trivial false statements on the ground that they can be paraphrased (for instance) as "All sets of equal things are easily annoyed", "All green things are hexagonal", but this type of paraphrase seems to me to ignore the way in which abstract words are actually used.) The distinction can, however, be marked by pointing to

¹ In his review of *Logic and Language*, first series, edited by Flew, this *Journal*, December, 1952.

the possibility of detailed criticism of the offending statements. If someone did seriously and literally uphold a statement of this kind it would be possible to argue against him; we might use as a working principle Parmenides' procedure against Socrates, by asking for elucidations and seeking to show that his view led to vicious regresses or other contradictions as a result of his attempt to treat abstract words as names of entities.

The same remarks apply to a number of examples of category mistakes given by Ryle in *The Concept of Mind*; for instance, those which arise from holding that team-spirit is a player, the University is a part of itself, the play has a part, that letters as well as words can be spelt, and from saying "He bought a right-hand glove, a left-hand glove and a pair of gloves". These illustrate a main class of category mistakes concerning collective words, or, more generally, the relation of whole and part. It might be hard to find people who would make statements of this sort, but if they are examined we find, again, that it would not be sufficient to say they are seen at a glance to break type-rules. To show that they are not empirically false statements, we should need to see how they could be defended, what other beliefs were associated with them, and then show that contradictions emerged. Thus, suppose someone asserted that "The University is the library", and when we pointed out that the library is part of the University, he showed that he was serious by accepting this but persisting with his statement. We could then see that he denied the very general truth that collections or wholes are not members or parts of themselves. Similarly, in the case of the gloves the point at issue would be the general truth that wholes are not physically additional to their parts, and in the team-spirit example the truth that properties of wholes are not parts of wholes; and similarly with the other examples. Thus, anyone seriously making one of the original statements would have to deny a general truth of this sort. Against such a person we could then argue that what he said was not merely false, but was mistaken in principle, because his denial of the general truth led to special logical contradictions; for instance, contradictions of the kind that Moore and Russell found in idealist accounts of whole and part. (I am not in this paper attempting to give a detailed account of the contradictions I mention; I am merely suggesting the lines along which I think they can be shown to emerge.)

There are various other general types of category mistake, obvious ones being those concerning time, space, cause, numbers, dispositions, but as a final illustration I want to say something

about relations, partly because they bear on certain views of Ryle, and partly because I think mistakes about relations are an important source of category mistakes. Moreover, there is a tendency these days to treat talk about relations with suspicion on the ground that those who do so often speak as if they are dealing with certain unnoticed, extra, features of the world. But, in my view, reference to relations can still be illuminating and need not lead to this sort of metaphysics, for the important things we want to say can all be stated by referring to the uses of relational expressions (even if in what follows, for brevity, I speak of relations some of the time in the material mode).

To speak of "relation" as a single category is somewhat misleading, for we need to make distinctions amongst a number of different kinds of relational expression. Some of these, of course, depend on differences in respect to symmetry, transitivity, number of terms, etc. Other distinctions turn on ambiguities which arise in discussing relations. Given that a complete relational statement is of the form "A has R to B", we may speak of a statement like "A is equal to B" as relational, or we may refer simply to the R, apart from its terms, as when we say "equal to" or "brother of" are relations. We may also speak of relational expressions occurring in statements in an elliptical way, as in "A is an R", "The RB is so and so", "R's are such and such", where we omit to specify one or both terms of a relation. For example, in statements like "George is a brother", "Peter's brother is asleep", "All brothers are males", by "brother" we understand the person or class of persons having the relation "brother of" to someone or other.

A large part of our vocabulary consists of relational expressions which are commonly used in this elliptical way; words like "employer", "captain", "lover", "survivor", "creditor", "king" and so on, are all of this sort. Tested by the sentence-frame method these expressions are found to function very often like ordinary object-words or quality-words. We can ask the same questions of brothers and employers as we can of men. And while in some cases we don't have special words for relational terms of this kind, we can speak about them if we wish; we can ask questions about things higher than, north of or next to such and such. So, in line with my earlier argument objecting to appeals to what most people strongly believe, a statement like 'All things higher than that gum-tree appreciate poetry' is in the same position as "Gum-trees appreciate poetry"; it is just a trivial falsehood. On the other hand, if we consider the complete relational statements in which these expressions can occur, it is

evident that they differ in category from quality-words and other object-words. We can say that George is a brother or is handsome, but while we can say "George is a brother of Peter", we cannot say "George is handsome of Peter". Likewise, we can say "A is next to B" but not "A is blue of B". (If this test for relational expressions were discussed in detail certain refinements would have to be introduced. For instance, while we can't say "A is blue of B", we can say "A is bluer than B", so that by this test all comparatives would count as relations too. Similarly, "Wisdom is a characteristic of Socrates" appears relational. These point to further distinctions that could be made.) The distinction in question is similar to those amongst dyadic, triadic relations, etc., and is summed up by saying that qualities (or, in Russell's language, monadic relations) cannot be treated as dyadic relations. With offending statements of this kind, grammar, or absence of sense, is often sufficient to indicate that a category mistake is involved; but, if necessary, the mistake could be demonstrated in detail by showing that the statement leads to contradictions. Thus an attempt to treat a quality-statement as relational would lead to an infinite regress. Suppose it were maintained that "The table is round" could only be understood by saying that the table has the relation "being round" to something or other (for instance, the Absolute). Some reason would have to be given for saying that we could not understand the first statement, and we could argue that for precisely the same reason it would be impossible to understand the second statement. So, in consistency, there would have to be a third statement like "The table has 'roundness to the Absolute' to a second Absolute", and so on for ever. Conversely, an attempt to reduce relations to qualities would lead to difficulties rather like those Bradley thought applied to qualities and relations in general. Given the view that, say, "on" as in "The book is on the table" was really a term or a quality, we could argue that if this conjunction of three terms were to be understood it would now have to be said that the book has a certain relation to onness and the table; *i.e.*, a new, triadic, relation would have to be introduced, and then, in consistency, a tetradic relation, and so on for ever. In other words, we could show that the distinction between terms and their relations is irreducible. (I may add that when relations are considered in the first sense I gave, as complete statements, they function rather like abstract words; there is little we can say about them without making category mistakes. Thus, if Melbourne is south of Sydney, it might be said that Melbourne's being south of Sydney is pleasing, sur-

prising or known, but not that it is red, round or interested in music. This illustrates the fact, not confined to relational statements, that only under very restricted conditions can a statement function like a phrase (or a proposition be a term in another proposition), and turns on the general distinction between asserting and referring.)

I can now refer briefly to an argument advanced by Ryle in *Dilemmas* (p. 10) when he speaks of those who accept Aristotle's ten categories as a sufficient number of pigeon-holes, and then continues: "Even in our own day there exist thinkers who, so far from finding this supply of pigeon-holes intolerably exiguous, find it gratuitously lavish; and are prepared to say of any concept presented to them 'Is it a Quality? If not, then it must be a Relation'. In opposition to such views, it should suffice to launch this challenge: 'In which of your two or ten pigeon-holes will you lodge the following six terms, drawn pretty randomly from the glossary of Contract Bridge alone, namely "singleton", "trump", "vulnerable", "slam", "finesse" and "revoke"?'. He then goes on to argue that each of these bridge terms is in a different category from every other, and supports this by maintaining that no two of them can be coupled in an "either, or" expression.

I agree with Ryle that there are not just two or ten categories, and I also agree at least that several different categories are represented by these bridge terms. What I want to dispute is his method of distinguishing them, and his implied view that none of them are relational expressions. Thus, in reply to Ryle's challenge, a hardened defender of qualities and relations might say that, apart from "singleton" and "vulnerable", which could be treated as qualities, these bridge terms are relations. This would not be a real answer to Ryle, because even if two of the terms can be called qualities, they are different from ordinary qualities since "vulnerable" is dispositional and "singleton" numerical, and because, as I have argued, "relation" is ambiguous and we have to distinguish different kinds of relational expression. Nevertheless, it seems to me true to say that four of the terms are relational. Briefly, "finesse", "revoke" and "trump" (as a verb) are commonly used either in complete relational statements like "North finessed (trumped, revoked with) the Queen of Hearts" (and if it is merely said "North finessed" we can always ask: What did he finesse?) or to refer to the abstracted relation itself, as in "Finesses should be made when the play for a drop is unfavourable", where by "finesse" we understand the various possible relations of finessing. On

the other hand, they may occur as elliptical relational terms. If it is said that the Ten of Spades is a trump or a revoke-card, we have to understand that it is a card in a certain relation, the other terms of which could be specified. (The same use could be made of "finesse" by speaking of a "finesse-card", but this is not customary.) "Slam" is different from these terms, but it illustrates another way in which elliptical relational expressions can arise. Just as, in place of a cognate accusative as in "He viewed a view" there may be a more specific accusative as in "He viewed a desolate scene", so in place of "He bid a bid" there can be "He bid a slam". And just as "bid" (as a noun) is an elliptical relational term which has to be understood as "that which has been or can be bid", so "slam", a qualified bid, has to be understood by referring back to the relation of bidding. (Similarly, in statements like "He made a slam", "slam" has to be understood as a result obtained from play.)

This is by no means an exhaustive account of these terms, but it reveals a number of distinctions denial of which could be shown to be logically inappropriate. For example, to say that North's finesse of the Queen of Hearts was a singleton or a trump (or red or round) would be a category mistake, for it would be to hypostasize the relation of finessing. But this procedure is rather different from Ryle's. His "either, or" test reveals a series of socially inappropriate questions, but it does not follow that they are all logically inappropriate. Thus we could, if we wished, ask whether North either revoked or finessed, but bridge players usually don't ask this question because they are not interested in it. Again, we could ask whether a card is a singleton or a trump; or, if the "or" is exclusive, it would be socially inappropriate to ask the question because it is known that a card can be both a singleton and a trump, but the question could be asked, for what is known is nevertheless empirical and capable of being denied. Similarly, it would be inappropriate to ask whether a card is a slam or a trump since we have no use for the question, but this is because we know the obvious truth that cards are not terms in the relation of bidding (or results obtained from play). To this the reply may be made that we are talking about the correct use of bridge terms and that anyone who questioned these obvious truths would show that he did not understand bridge; in other words, that the true statements which can be made about bridge are just what reveal the special categories of bridge. But this is the test for categories I criticized earlier. Applied consistently in all fields, it turns any statement generally accepted as false into

a category mistake, and fails to give an account of distinctively philosophical mistakes.

To conclude, what I have been arguing is that if the concept of category mistakes is understood loosely it can lead to a confusion between social and logical inappropriateness, or, what comes to the same thing, it can turn the difference between obvious empirical truths and falsehoods into indubitable philosophical differences. When this is done there is an undue multiplication of category differences; each special field of knowledge contributes numerous categories of its own. So I prefer to speak of philosophically interesting categories and say that, while there may still be a large number of categories, they can have a general application; an expression in a given field may belong to the same category, or occasion the same type of category mistake, as expressions in various other fields. Such a qualified account can be given by referring to the contradictions statements may lead to. To do this, however, involves examining the assumptions behind any given statement and being able to show by detailed criticism that it does lead to contradictions. In other words, to find what appears to be an odd or absurd use does not so much solve as present the problem of showing that some basic confusion of categories is involved.

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DISCUSSION

THE PERFECT GOOD: REPLIES

By C. B. MARTIN

I shall treat the criticisms of Mr. Grave, Mr. Franklin and Mr. Smart¹ separately.

I

The following is a quotation from Mr. Grave:

"It [Mr. Martin's argument] gets its plausibility from the suggestion (half-made by the words themselves) that the reason why 'nothing could count as evidence' against God's goodness is that we do not know what would count. We know very well what would count. If God damned an innocent man that would count. To say that God is by nature morally perfect is just to say that God not only does not do, but could not do the things that would count against His goodness; that He could not do the sorts of things which are evil, and which we would call evil; that He not only does, but must do, the things that are good and that we would call good."

The weakness of Mr. Grave's argument may be brought out by considering the parallel case offered by St. Thomas. St. Thomas takes the statement "Man is a donkey" to be a contradiction in terms parallel to "God does evil". (*Summa Theologica*, Q. 25, Art. 3.)

The following is a play upon the quotation from Mr. Grave. Someone might say the reason why nothing could count as evidence against man's not being a donkey is that we do not know what would count. We know very well what would count. If man had donkey's fur, ears, legs, that would count. If man had donkey's tail, eyes, jaw, propensity to give birth to donkeys, that would count. To say that man is by nature not a donkey is just to say that man not only is not, but cannot be the things that would count against his not being a donkey.

Also, concerning evidence against the statement "A circle is not square", we know very well what would count. If a circle were composed of four straight lines, that would count. If a circle were a four-sided figure, that would count. I think this makes parallel nonsense.

¹ This *Journal*, August, 1955, and (for Mr. Smart) December, 1955.

Mr. Grave has to establish a sense in which there can be contrary evidence against a statement whose negation is a contradiction in terms. I do not see how this is to be done. It requires more than the introduction of a form of words. Mr. Grave's instances of contrary evidence are of no help, for the supposition "If God did nothing for the happiness of men" is itself a contradiction in terms parallel to "If a circle were composed of four straight lines" or, if it is not, then it describes a logically possible state of affairs and evil becomes predicable of God even though He does no evil. If it is a contradiction in terms and describes no possible state of affairs then its negation is true in all possible states of affairs and so vacuously true, for it is true no matter what happens. Mr. Grave illustrates but does not resolve the dilemma.

Mr. Grave's further criticism concerns what I took to be a contradiction in the Incarnation, namely that "Christ can be conceived to have been other than He was, that is, not good; yet, as God, it should be inconceivable that He should have been not good".

Mr. Grave says "Mr. Martin will allow orthodoxy to keep the two natures of Christ 'unconfused'. Then there is no contradiction. The God who is also Man is necessarily good. The Man who is also God is necessarily good, but He is necessarily good not because as man He could not fail, but because it is impossible that God should have become incarnate in Him without foreseeing that He would not fail."

This, however, does not solve the problem. Even if God infallibly knew that Christ as man would not fail, still as man He could have been otherwise than, as God knew, He in fact would be. It is not that "the Man who is also God" is *good necessarily*, but that God knew with necessity that the Man would *in fact* be good.¹

God's necessary goodness is of a totally different kind. It is not only that God knows with necessity that He (God) has been, and ever will be, good, but also that as God he cannot be other than good. Evil is not predicable of God. Neither is four-sidedness predicable of a circle.

The problem of the Incarnation is the general problem of the expression of the Divine Perfection. If the divine nature is that about Christ that could not have been otherwise than good, *what* is it about Christ? Every word, action and disposition of Christ we can conceive to have been otherwise (evil) than it in

¹ It is obvious that "knows by necessity" has difficulties parallel to "good by necessity".

fact was (good). It seems that the only way to avoid contradiction is to keep the Divine Nature from all expression, but then it comes to nothing.

II

Mr. Franklin agrees with me that sometimes (as in St. Thomas) "God is good" expresses a necessary proposition. (Of course, a good deal of my argument had to do with showing the subtle unsteadiness of use of the sentence "God is good".) But he resists my conclusion that this necessary proposition is vacuously true.

The following is a quotation from Mr. Franklin:

"I will approach the question by suggesting that not all necessary propositions are vacuous. Some obviously are: for example, the well-known one 'Nothing can be red and green at the same time all over'. But there are also such propositions as 'Water boils at 100°C. at atmospheric pressure'.¹ This is usually, and I think rightly, said to be necessary too. No doubt it started as an ordinarily empirical generalisation. But we have such very good evidence for it, and none against it, that now we, or at least the scientists, have made it part of our definition of water. So if we find a liquid just like water except for its boiling point, we (or at least the scientists) say not 'this is very queer water' but 'this is very queer stuff, just like water except for its boiling point'. That is, we have now certified this former empirical truth linguistically, and have thus, as it were, given it an *ex post facto* necessity.

"But though it is now necessary, it is as non-vacuous as when it was an empirical generalisation. For although nothing could falsify it so long as we kept the definition, we can imagine cases where the definition would be altered."

I believe that Mr. Franklin is confused about (a) the nature of propositions and also about (b) the nature of necessary propositions.

(a) Mr. Franklin says, "But though it is now necessary, it is as non-vacuous as when it was an empirical generalisation."

What is this "it" that changes? A form of words can change, but this is trivial, and anyway in this case they do not. Our use of a given equivalent form of words can change.

¹ This is an unhappy example since 100°C. was defined in terms of the boiling point of water. Otherwise such an agreeably round number describing such a fundamental event might be considered material for the Argument from Design. However, for simplicity's sake I shall retain the example.

That is, our use of "Water boils at 100°C . at atmospheric pressure" or any equivalent form of words may change from a use such that we express a contingent proposition to a use such that we express a necessary proposition. This is like the speed of an object changing from one speed to another. Here there are two senses of "use" and "speed". In one sense of "use of a form of words" there can be many different uses, as with one sense of "speed of an object" there can be many different speeds. The speed can change from 80 m.p.h. to 85 m.p.h. The use can change from a use such that a contingent proposition is expressed to a use such that a necessary proposition is expressed. In the other sense of "use" there can be not more than one use and so no change, as in the other sense of "speed" there can be not more than one speed and so no change. The speed of 80 m.p.h. of an object cannot change and remain the speed of 80 m.p.h. The use such that a particular proposition is expressed cannot change and remain the use such that the same proposition is expressed.

It is this second sense of "use" that comes close in meaning to "proposition". Perhaps the above-mentioned ambiguity is what leads Mr. Franklin into the error of speaking as if change is admissible of propositions.

(b) Mr. Franklin argues for his *ex post facto*, non-vacuous necessity by pointing out that we may have come to express a necessary proposition after having expressed a contingent proposition by the same form of words, and also by pointing out that future events can incline us to cease expressing a necessary proposition by a given form of words. This, I think, is all that Mr. Franklin's argument for the non-vacuity of certain necessary propositions comes to.

Let us call "Water boils at 100°C . at atmospheric pressure" P.

So far as P is concerned, it is true no matter what happens and so is vacuously true. Of course, if certain circumstances had not obtained in the past, then I should not have learned the concepts and so not been able to express P. So, because I can express P it seems that we can work back to what *was* in fact the case. However, what was the case *need* not have been water boiling at 100°C . I should just have to have known about water (perhaps as a colourless, tasteless, drinkable liquid, found in fishponds) and known about liquids (perhaps other than water) boiling and known about 100°C .¹ In order to be able to assert proposition P, water need never have boiled at 100°C . Proposition P *could be as it is* whether "derived" from a true or false

¹ If, for the sake of coherence, anyone wants to change the case to the melting point of iron or something comparable, he is free to do so.

empirical generalisation or not derived from a generalisation at all. Someone, perverse or ignorant, could arbitrarily define water as boiling at 45°C. or 100°C.

You can say "But something had to be the case for us to learn the concepts with which to formulate proposition P". No doubt. And, of course, there is a parallel here with red and green all over and $2 + 2 = 4$.

We may come to express the mathematical proposition $2 + 2 = 4$ after taking this form of symbols or words to be the empirical generalisation that when you add two things to two more things you can then count four things. This does not, I think, establish the non-vacuity of $2 + 2 = 4$.

If "God is good" is to be parallel to proposition P as a necessary proposition then I should resist in the same way claims for its non-vacuity.

The emptiness of the two statements is obvious when they are clearly formulated.

(1) Water (odourless, tasteless, liquid, and boils at 100°C.) boils at 100°C.

(2) God (creator of all things, omniscient, omnipotent and good) is good.

It is important to note that "water" and "God" are here used descriptively, and not as names. (P. T. Geach in "Form and Existence", *Aristotelian Society Proceedings*, 1955, claims that Aquinas used "God" as a descriptive term and not as a proper name.)

Mr. Franklin says "The questions about His perfection are converted into questions about His existence".

Since "God" is a descriptive term defined partly in terms of goodness, we have defined away the question of whether or not what we call "God" must be called "good". The problem now remains as to what sense can be given to the question "Is there a Being to whom the description 'God' applies?". Mr. Franklin's treatment is here so schematic and the problems raised so wide that discussion would be unprofitable.

However, if "God is good" is to be a necessary proposition and not a rich muddle of conflicting uses, then it is important to see how little comes of it.

All that is necessary (and this by definition) is that where the term "God" may be applied it follows necessarily that the term "good" may be applied.

From this it does *not* follow that the individual truly described as "God" should be necessarily good, in a way such

that it would be inconceivable that He should be other than good, or anything else than that He is in fact good. Theologians have regularly wished to have the goodness of God as an individual somehow necessary. But if "God is good" is a logically necessary proposition, then the necessity does not concern any individual or any of his properties. The only necessity is the barren one concerning the use of the terms "God" and "good".

Of course, logical necessity is not the only kind of necessity, and I do not wish to assert that God's existence or goodness can have *no* kind of necessity.

For example, we may conceive of God's existence as necessary in the following way:

God may be conceived of as

1. A being for whose existence nothing else need exist.
2. A being that has always existed.
3. A being upon whom everything else depends for its existence.

If there were such a being His existence would be necessary in two ways:

- (a) He would not be contingent (*this* sense of "contingent" has to do with things and not propositions) that is, He would not be dependent upon anything else for His existence. Necessity here comes to cosmic self-sufficiency.
- (b) His existence would be necessary for the existence of anything else.

This sort of necessity does not rule out the following:

1. This being does not exist.
2. This being exists, but it is conceivable that He should not have existed.
3. This being exists and will cease to exist.

I should be suspicious of a necessity that would pretend to rule out any of these.

We may conceive of God's goodness as necessary in the following way. (Doubtless there are many other ways.)

Some things can change and affect little else. God is not like that. God's goodness makes things not so bad as they might be. If God were to become evil, things would be absolutely hellish. If one can both give meaning to and truly assert the hypothesis that God exists, then to conceive of God's being otherwise than He is is no small matter, for it is to conceive of cosmic alterations in the way the world goes. That the law of gravity should

cease to have application would have results of no greater generality. We tend to think of what is basic to the nature of the world as being more necessarily as it is than is something not so fundamental to the world.

Also, of course, there are countless reasons why men do evil that would not apply to God. For example, since God knows all, He cannot be victim (as can men) to unexpected temptation, nor can He lack insight into the nature of His actions. Self-deception, a real force for evil in men, would not be present in God. It is such non-self-contradictory and non-obscure facts about His nature that would give sense to the necessity of His goodness, and that would really set Him above His creatures. This is to make God a moral giant whose necessary goodness is different only in degree from that of men.

III

Mr. Smart divides his criticism into two parts.

In the first part he criticizes my assumption that the characteristic use of the notion of God's Will "is found in the *justification* of moral decisions, and so has a mainly deliberative foundation", and suggests another employment of the concept.

In the second part he tries "to show that it is not impossible by *definition* that God should be imperfect, but impossible in a different way".

- I. "One of the functions of prayer is to maintain these dispositions [humility and resignation] as well as to express them. Now, on the supposition that those who have these dispositions find it easier to come to the correct moral decisions, the claim that prayer helps one to discover one's duty would not be implausible. So the sharpest use of such an optative sentence as 'Thy Will be Done' is (apart from its expressiveness as praise) to put one in an earnest humble and resigned frame of mind before tackling a moral problem. Appeal to God's Will, on this view, does not provide reasons for a choice, but is a way of exhibiting the manner in which certain religious dispositions help men to reason in morals more dispassionately. It has, as it were, a formal and not a material relevance to moral problems. To turn to God for 'advice' is not really so to do (as it is with Father), since no material advice is given."

I do not think this account is consistent with the main stream of Christian belief and practice. Mr. Smart says "prayer

is no substitute for moral reasoning", and there is a sense in which this is true. One should not pray *instead* of reasoning. But sometimes when the religious person has thought as hard and as honestly as he can he still may be undecided as to what he should do. He may then pray to God for guidance. As a result of prayer he may feel that God has made known to him His Will. It is not necessary that he should seem to hear God speak or see a dazzling light or in any way come upon a Divine sign. He may just feel confident after praying that one course of action is right, and others wrong, and he may feel that this confidence and decision come as a result of God's action and guidance.

It is essential to prayer that "He that cometh to God must believe that He is". On Mr. Smart's account of prayer where he says "So the sharpest use of such an optative sentence as 'Thy Will be Done' is (apart from its expressiveness as praise) to put one in an earnest, humble and resigned frame of mind before tackling a moral problem", it almost seems that an atheist, with consistency, could pray and think of prayer in this way. Certainly nothing about God's action is implied in this analysis of prayer, though a great deal is implied about the psychological benefits of prayer.

I wonder if Mr. Smart is an atheist? Even an atheist can use a religious form of words. It all depends upon *how* he uses them. I feel very uneasy about Mr. Smart's use.

Consider the following quotation in this context:

"Strictly, it is true, we might have thought that *union* with God should be the *summum bonum*; but in fact calling God this is not unnatural, inasmuch as 'experience of God' functions much like 'experience of bliss'. And mystics speak of seeking God, as in a way one might seek happiness."

"Not unnatural." It is positively un-Christian and untheistic! Mr. Smart's use of "God" here is one that could perfectly well be employed by an atheist with a taste for religious forms of words and practice. No atheist who denies the existence of God need deny the existence of "bliss" or "happiness". It would be odd, I think, to talk of a happiness creating the world and of bliss "forgiving sins". It is not that Mr. Smart's use is a partial but not complete use of "God", but that it is a use inconsistent with traditional use. It is, indeed, atheistic.

2. Professor J. J. C. Smart gives an account of the necessity of God's existence that can be made to hold also for God's goodness.

"Theological necessity cannot be logical necessity. In the second place, I think I can see roughly what sort of necessity theological necessity might be. Let me give an analogy from physics. It is not a *logical* necessity that the velocity of light in a vacuum should be constant. It would, however, upset physical theory considerably if we denied it. Similarly it is not a logical necessity that God exists. But it would clearly upset the structure of our religious attitudes in the most violent way if we denied it or even entertained the possibility of its falsehood."¹

Mr. R. N. Smart makes what is in effect an application of this account to the necessity of God's goodness.

"Since the meaning of the expression 'God' is largely determined by a fair number of statements in which it occurs (these being the more important doctrines of the theological system), it is not obvious that some of them may not be jettisoned without disqualifying us from using the expression in the other statements. Yet it could be objected that people would have a strong resistance to jettisoning the statement 'God is good' without also abandoning the use of the expression elsewhere . . . Expressions of wonder involve the ascription of sublimity and marvellousness to their target. In a monotheistic religion all this is concentrated upon the Creator of the world, to whom is thus ascribed what we may call supreme wonder-value. . . . the notion of sinfulness combines different inadequacies, the most important being moral culpability; and God's wonder-value is expressed as the converse of sinfulness—as holiness, including goodness. Thus it becomes something like a theoretical requirement that God should be good . . . Yet it is still sensible to deny this statement and without self-contradiction, *provided* it is recognised that the composite system would have to be sacrificed. Whether the system (including this requirement) is true or not depends above all on the evidence, and it is still open to us to choose another system or none at all . . . The moral of all this is that the more deeply a requirement is written into a theological system, the more it looks like a definition. It becomes more heart-rending to allow an infringement; but this resistance does not lead to vacuity, because the requirement is related to the rest of the system, and the latter need not be vacuous. And a theoretical (or quasi-theoretical) impossibility is not a logical impossibility."

According to this account, God is not *defined* as "the supreme wonder-value", but He is *described* in this way. Because, of course, if God were *defined* in this way it would follow that He was *defined* as "good", for "goodness" follows from "supreme

¹ J. J. C. Smart, *Church Quarterly Review*, April-June, 1955, at p. 188.

wonder-value". Thus, the statement "God is good" would be true by logical necessity. Whether there was a being to whom the concept "God" could apply would be another matter. This would amount to the same thing as the view discussed in the previous section.

However, even if God is described and not defined in this way, God cannot be described as "supreme-wonder-value" if He cannot also be described as "good". It is necessary—logically necessary—that if God is not good then He is not supreme wonder-value, for part of what is meant by "supreme wonder-value" is "good". If "God is good" is false then it follows by logical necessity that "God is supreme wonder-value" is also false. It does not follow that "God is the Creator of all things" is false. Perhaps this is what Mr. Smart means by a requirement being "written into a theological system". How many descriptions you can whittle away and still be said to employ the same concept is not always easy to say.

I don't think that it could be denied that the notion of God is defined in terms of what is allowable within the theological system. Now, if there is (as Mr. Smart implies) some part of that system that is so essential that if that part is scrapped the whole system must be scrapped, then it would seem that that part ("God is good") *is essential to the notion of God*. This indeed "looks like a definition" and I am at a loss to see how it is not one.

But there is a kind of necessity involved in all of this that is not logical necessity. I might be unwilling to give up saying God was good because it would be upsetting or "heart-rending" to do so. It is somehow necessary for me to believe that a good God exists. If I gave up this belief I should have to give up a whole way of life. This attitude is both repellent and attractive. I leave its further treatment to moralists and psychologists.

It is obvious that whatever sorts of necessity there may be in this account they do not apply to God or His nature. They apply either to the use of statements or to the user of those statements. I believe that traditional theologians have wanted the necessity of God's goodness to be something about God.

Finally, there is an argument that I can hardly believe that I understand. "Nevertheless, it may well be that the impossibility of God's erring is not a logical one (induced by our commitments as to how to use the expressions 'good' and 'God'). There are one or two things which cause me doubts on the matter. For example, the expression 'God' occurs in a very large number of

different (some of them very different) statements, which do not hang together tautologically; thus 'God created the world' allows us to define God as the Creator of the World, and it is not self-evident that the Creator is good."

This, if I understand it, is nonsense. Not everything that is true by definition of a concept entails and is entailed by everything else that is true by definition of that concept. I can so define "platypus" that "webbed feet" is true by definition and "suckles its young" is true by definition. Neither entails the other.

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WHAT IS A PROPER USAGE OF "ILLUSION"?

By P. Æ. HUTCHINGS

Mr. Armstrong's article "Illusions of Sense" (this *Journal*, August, 1955) and his discussion on Berkeley on hot water (*Analysis*, December, 1954) have provoked the remarks that follow. I think Mr. Armstrong's defence of naïve realism deserves all our sympathy. The plain man (e.g. the student as we find him at first, uncorrupted), S. Augustine and the post-Wittgensteinian all feel that there is something very wrong about epistemology. As a plain man and a Wittgensteinian I am with them.

I

Mr. Armstrong remarks in his *Analysis* article on Berkeley and the hands hot and cold in the same water: "I obtained a hint of the solution of this puzzle from Professor J. Anderson (cf. *A.J.P.P.*, 1929, p. 144) where he suggests that in the context of this puzzle 'hot' may mean 'hotter than my hand'."

Of course it does. Professor Anderson and Mr. Armstrong are quite right, but this is the point surely about the spuriousness of most epistemological puzzles.

They arise from

- (i) the basically queer notion of trying to define something, for example a table, in terms of something else of a quite different sort, for example sense data. And from
- (ii) the resultant hopeless necessity of reconciling the absolute paradigm existents. In Berkeley's puzzle we have the "absolute hot" and the "absolute cold" sense data which do, utterly, conflict.

In fact neither the problem of "the illusoriness of everything" nor the "sense data as paradigm existents" 'way out' is logically viable: the construction of the world out of just those sense data whose 'incoherence' led to the "is it all an illusion?" question is, as we shall see below, the final paradox.

Mr. Armstrong, though, does not seem to see the full force of this. He thinks that it is proper to say that something, for instance a mirror image, is "less real" than something else:

"It is very natural to say that he who looks in a mirror is perceiving *something*, but that, since there is *really* only a pile of books behind the surface of the mirror, this something is¹ *only an appearance*." (A.J.P., August, 1955, p. 91.)

It is not natural at all. I say only to very young children "that's *not really you*, only you in a mirror" because it's only very young children who have not learnt the facts about mirrors; they are puzzled and want the playmate on the other side (so do kittens) and theirs is the only legitimate puzzle about mirrors; but it is a soluble puzzle, and the philosophical one is a spoof. The "less real" talk, if it is to mean any more than "it's not really you" is vicious.²

II

Perhaps we can put it this way:

There are two things the matter with epistemology: in the first place what it treats as a problem, "How to show that all is not illusion", is in fact a pseudo-problem; in the second place its efforts at construction of the world are not what they seem to be at all; as soon as they succeed they are not "philosophical" but just commonsensical. The epistemology that does work is the one that treats the world, covertly, as something about which no epistemological puzzles arise.

On the first point I don't intend to say anything; for those who disagree I suggest a short meditation on S. Augustine's *Contra Academicos* (Bk. II, ch. 7 and ch. 10, pp. 85 ff. and 126 ff., O'Mera's translation, Longmans, 1950) and Wittgenstein's *Tractatus*, 5, 64.

On the second point: whatever "doubts"³ we can have as a result of contemplating mirror images and oars in water we can't come to think "all may be illusion" because there is nothing that would be "all being illusion" nor could there be. The doubts we construct do tend though to this impossible position; but they

¹ This is, also, another version of the Bradleian sort of confusion over the "is" of existence and the "is" of predication.

² "The World is everything that is the case." Wittgenstein. *Tractatus*, 1. We cannot restrict "real" *a priori* to material objects—if we play that game—or to sense data, if we play that one. Whatever turns out to be real, is.

³ Wisdom's point is that they are not "real" ones. Descartes "doubted" as part of the method, that is, he wanted an excuse to reconstruct the world on an—inappropriate—mathematico-deductive model.

are I think spurious doubts, just in so far as this is the sort of pointless master doubt to which they lead.

What "worries" us—philosophically—about pink rats and oars in water is their unlikeness to "ordinary" phenomena. Pink rats are not like the brown ones in corn silos: oars in water are not like the ones in the bottom of the boat. This extraordinariness can worry us for two reasons:

- (a) just because the business *looks* odd;
- (b) because we want to construct the world out of some simple, given, and irrefutable things—mistakenly believing this to be necessary—and sense data are the only candidates.

The point about (b) is that sense data "quarrel". Hands are hot and cold at once in the same wash basin, I blow on porridge to cool it and on my hands to warm them—and all the usual examples. The point about (a) is that the admitted oddness of the pink rat has allegedly led people to think that the world *needed* construction by philosophers. This is a fine impasse.

There are ways out of the oddness of bent oars, pink rats, and distant towers smaller than match boxes which are available to the non-philosophical, but apparently the philosopher is not allowed to use them. He does though.

Russell in one of his constructions of the world—"objects are logical constructions of sense data"—talks about "perspectives", and Mr. Armstrong in his article in the *Journal* talks about "square shape".¹ What they are both doing is using an art student's model of the world; but somehow this is supposed to be philosophy. I do not see the point at all. Mr. Armstrong's "grid" held up to the world has a perfectly good practical use and Dürer used it, or at least suggested its use, for figure drawing. If you want to draw, that is, reproduce "square shape" on paper to give the "illusion" of "real shape", make a frame and stretch strings across it and draw what fills each square made by the strings.² The fact that it is drawing technique makes me at least suspect that its relevance to philosophy is other than Mr. Armstrong would have it.

What do some people want to call "illusions"? Mr. Armstrong's suggestion, as I understand him, is that the tower-small-in-the-distance can properly be called an illusion. Perhaps so. What are the reasons for doing so? Perhaps somebody

¹ *Op. cit.*, 96.

² Cf. for instance the diagram for this in *The Artist at Work*, H. Ruhemann and E. M. Kemp, Penguin Books, 1951, p. 21.

might be misled into *saying* that as the matchbox and the tower *look* the same size, *they are* the same size. If he means by "the same size" just "looking the same size" this is a possible but eccentric use, but we mean usually by the "same size" something different: e.g. mass, weight, etc. etc. As long as we talked about visual sense data "the same size" would be plane size—if we wanted this usage. The difficulty of course is to "reconcile" the "disagreements" among visual and visual, and visual and tactile, sense data: nobody seriously doubts about the size of towers as a rule—it is the occasional optical illusions we are taken in by, chance ones or conjurers' ones or *trompe d'œil* we pay to have painted. In real life we know what to do about "toy" motor cars that approach us on the highway. They get big enough to give us a nasty jolt quite quickly. But when we use the "philosophical" theory of perspectives what are we doing?

If we want to call pink rats and mirror images "illusions" or even "things-less-real" we may do so: but if we think we can translate them into some philosophically neutral thing like sense data, which are ideally homogeneous, we find we cannot. Sense data are not as sense data homogeneous by any other token than by their being sense data. To get a world out of them we have to come to terms with their heterogeneity by the corrective admission of "perspectives" etc. But then we have broken into ordinary non-philosophical "realistic" talk. In it we have the model that reconciles sense data, and the "unphilosophical" usage of "real" which will admit images, shadows, tables and thoughts all to "the class of real things"

What is the point in calling a mirror image "only an appearance"? We may draw attention to the fact, quite unnecessarily in most cases, that I am *not* face to face with my *doppelgänger*, but we can *not* successfully exploit the quasi unreality of mirror images, as opposed to the reality of me, to make a philosophical puzzle we can then triumphantly "solve". Common usage is already quite in order: we think of quite different things, as I have already suggested, as real—thoughts and cabbages—but we say "x is not really y" when people are ascribing "the wrong sort of reality" to things: "it's not really another kitten, just a reflexion of Felix." If we will admit only sense data as real, even in some quite provisional way, and then try by epistemology to "get at" the common-sense world, the best we can do is to construct a "philosophical" and spurious set of doubles of the ordinary or scientific models that we use in non-philosophical moments for dealing with mirrors, oars in water or pink rats. When I say "image in a mirror" I don't expect in real life that

"image" will mean the same as "retinal image", "*direct* images had by me now", any more than I expect it to be the same as "graven image". "Illusion" can be used to make out the differences that do exist between sense data; as soon as we take the word too seriously we generate puzzles of "more real and less real" and of "reconciliation". There are puzzles but not philosophical ones. I am puzzled by some optical phenomena because I am too lazy to go and find out the model which would dissolve my puzzlement—but I would find out from the science people and not from philosophers. And I am certainly no wiser when I am given a "philosophical" ghost of the ordinary explanations—e.g. when I am told about philosophical "perspectives".

There are two "aphorisms" of the Aristotelian tradition that are worth brooding over in connection with this; one is, "the senses are infallible", and the other is, "mind is in a sense all things". As long as we are *really* naïve realists, or utter Aristotelians (and Augustinians or Wittgensteinians) we are safe from epistemology. As soon as we are Cartesians, Humians, etc., we are committed to epistemology and as far as I can see it is a maze that we cannot, in the nature of the thing, ever hope to get out of.

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CRITICAL NOTICES

THE MORAL NATURE OF MAN. By A. Campbell Garnett. New York, Ronald Press Co., 1952. 278 p. \$3.75.

RELIGION AND THE MORAL LIFE. By A. Campbell Garnett. New York, Ronald Press Co., 1955. 223 p. \$3.50.

These works, written by an Australian who is now a professor of philosophy at the University of Wisconsin, form a natural pair or sequence. Proceeding respectively from a study of morals and a study of religion, they affirm, taken together, an ethical and religious liberalism founded on the moral consciousness of man. Their main theses are as follows:

(1) All moral obligation is to "produce good consequences". (M.N.M., p. 132.)

(2) To say that something is good is to say that it is "reasonable to adopt a favourable attitude towards it". (M.N.M., p. 120.)

(3) It is reasonable to favour any course adapted to the structure of human nature.

(4) Man is so constructed that his "feeling-striving processes are primarily and basically disinterested". (M.N.M., p. 152.)

(5) His sense of personal integrity is therefore bound up with there being in existence the greatest total amount of good, whether or not it corresponds to his immediate inclinations.

(6) It cannot be satisfied by his *own* pleasures, or by the satisfaction of his *own* desires, or even by his *own* self-realization.

(7) At the same time these things are such as, in principle, it is reasonable to favour, as "nature" commends them; though, according to the axiom of disinterestedness, they must be favoured irrespectively of our own concerns.

(8) "Conscience" is what guides us in discerning the "greatest good" in any particular instance. It is "critical" and heuristic. The "traditional" conscience which adheres to rules belongs to a different order.

(9) "Moral good" is related to "natural good" in that it "comes into existence in the effort to produce natural goods". (M.N.M., p. 237.) For that reason it can be neither a constituent of natural good nor a means towards it. In itself it "consists in the effort to do what one thinks is right". (M.N.M., p. 149.)

(10) To follow out one's moral obligation there is required a "unique moral effort" (M.N.M., p. 207), directed to "a large and more rational perspective" (M.N.M., p. 225). This is what is meant by "free will".

(11) Throughout, Professor Garnett wishes it to be noted that "all ethical terms, even the term 'obligation', have been defined in terms referring to natural psychological processes" (M.N.M., p. 165).

These are his central moral theses: and they are independent of his further theses on religion. For it is one of his main contentions that religion is an outgrowth and unfolding of morality, and not the prerequisite or condition of morality. He first declares morality to be independent, and only then proceeds to show that the premisses of morality force us on to religion. With that warning we may pass to the theses on religion.

(1) "Morality is the very foundation of religion" (M.N.M., p. 241). "Religious views are logically dependent on the ethical views held, and not *vice versa*" (R.M.L., p. 14). He thus denies both that morality depends on religion, and that religion depends on anything other than morality.

(2) Religion is broadly defined (R.M.L., p. 6) as "an attitude of devotion to something other than the self which is regarded as worthy of supreme devotion". The nerve of the definition is in the word "worthy", which excludes from the start devotion undirected by a sense of excellence.

(3) Though humanism is a religion within the meaning of the definition, it is not a complete religion, both because it cannot absorb our unconditional loyalties without contracting them (R.M.L., p. 90), and because it cannot provide the inspiration needed for the fulfilment of its own ethical programme (M.N.M., p. 251).

(4) It thus emerges that it is not in prescribing standards of excellence, but in making it possible to attain them, that religion makes its great contribution to morality.

(5) Any religion is a matter of faith and not of certainty: Garnett draws the distinction with an unusual sharpness. Logical demonstration in these fields is not possible; if it were, religion would lose its essential element of risk and degenerate into other-worldly prudence. (R.M.L., p. 121 *et passim*.)

(6) On the other hand, though faith is "not dependent on the positive support of reason", it is "not independent of the negative criterion of reason" (R.M.L., p. 124). "Its optimum

value is maintained" if it can be shown to have a "modest degree of positive probability" (R.M.L., p. 125).

(7) And faith has "an essential intellectual content"—the love of God for man, of man for God, and of man for man (R.M.L., p. 117). For the rest, however, "its ideational implications are unessential, tentative, and few" (R.M.L., p. 149).

(8) The evidence which makes faith reasonable is experiential and ethical. Historical revelation has to be reflected in experience to produce its effect (R.M.L., p. 179), and the evidence of nature, apart from morality, is ambiguous and inconclusive.

All these seventeen theses could be debated at length. The most a reviewer can do is to discuss a selection. And it seems appropriate, in the present state of ethical discussion, to begin with the last thesis in the first series. What the writer here says is that ethical terms can be redefined in terms of "natural psychological processes". He commits the "naturalistic fallacy" openly, consistently, and comprehensively. If his procedure can be sustained, a consideration which, more than any other single factor, has in our times changed ethics from a philosophy of morals into a series of logical exercises on the subject of morals, will have been suitably disposed of. But there are difficulties.

As has often been pointed out, even if "good" cannot be *defined* in terms of e.g. human welfare, it can still be maintained that "human welfare" is, *as a matter of fact*, the only and sufficient object towards which it is "reasonable to adopt a favourable attitude". But Garnett is not satisfied to proceed in this way. It would require him, for one thing, to adopt the historical approach, and to show how in all their various advocacies all ethical writers, and all moral agents, assumed from the start, or inconsistently admitted in the course of argument, that good coincides with human welfare: whereas his method is psychological in the old sense of the word; he appeals to the structure of human nature. In so doing, he not merely notes a coincidence: he invokes a necessity. Human nature being what it is, what it irrevocably is, the good life consists in recognizing it and following it. Here we have not merely an evasion of the argument of the "naturalistic fallacy": we have a straightforward denial of it. And this is difficult, because, as Moore showed in a similar case, it would still make sense to ask, "But is human welfare good?", and if the terms were interchangeable, it would be a contradiction.

The point is important, because it affects classical writers like Plato and Butler as well as Garnett; and it is particularly

profitable to watch Garnett's procedure. In all three cases it is clear that, where there is reference to human nature, what is really being said is that there are certain standing characteristics of human beings which are ignored at one's peril. But it is particularly clear in Garnett, and that is why he is so instructive. When he says that the "feeling-striving processes are primarily and basically disinterested", he means, and shows that he means, that to act otherwise is sooner or later to be a failure. The "nature" of man is not a concealed mysterious "I know not what"; it is disclosed in the plain incompetence of other courses. There is, in fact, a test for "nature": and it is more of an empirical test than he himself realizes. This, perhaps against his will, Garnett may have to admit. If he does, his appeal to "nature" can be reasonably defended.

Empirically speaking there are still difficulties; these are discussed later. But the principle, that ethical questions cannot be determined without reference to existential questions, that there is some element of "is" about every "ought", has been reaffirmed with conviction and persuasiveness. On this point two observations may be ventured. The first is that something of the kind is quite necessary if there is to be any tie-up between morals and religion. The second is that it is remarkable how fashions return. By merely sitting tight and thinking from the premisses of the 1920's Garnett finds himself in the company of contemporaries who are just straggling back from their adventures in the intervening wilderness.

But, passing from method to detail, there are still problems. A minor problem, often overlooked by writers of Garnett's type, and in fact not noticed by Garnett himself, is whether at all times and for all purposes the well-adjusted character has more to give than the character in conflict. It is at least arguable that for sheer productiveness in certain fields a decent minimum of frustration is a positive asset. At the very least the question is one for empirical study. "Self-realization" should not be accepted out of hand, as it was in the 1920's, because it enabled post-Hegelians to consort with amateur students of psycho-analysis.

More important, however, is the question whether the total amount of good in the world (however good is understood) is the only ethical consideration. The standing difficulty about such a view is that there are other ethical considerations based on the idea of justice. Garnett sees the difficulty and tries to meet it, but, in the reviewer's opinion, not successfully (M.N.M., p. 171-2). What he says is that to neglect these considerations is to admit an "inconsistency in the volitional life", which "threatens

the self's own need of inner integrity". But, if only the total amount of good is morally significant, why should we worry about "inconsistency", and why should our "integrity" be disturbed? It does credit to the author's moral insight that he should feel that way, but it wrecks his claim to a single criterion. He claims that his account of the matter "shows the common source of both kinds of obligation", and is thus an improvement on that of Ross, who merely exhibits them side by side. But, unless the argument escapes me, there is nothing in common between the two situations as Garnett presents them except that in both cases one's "inner integrity" is at stake. And in that case the total amount of good in the world is not the only ethical consideration.

A third reservation which occurs to the reader relates to Garnett's account of "moral good". In insisting that "moral good" comes into being on the occasion of the effort to produce "natural good", he is obviously indebted to Hartmann ("morality comes in on the back of the deed"), and Hartmann's ordination of the virtues is quite incompatible with the reductive teleology of Garnett's early criterion.¹ Either "good" includes "moral good" or it does not. If it does, then there is one kind of good which is not itself human welfare but is only evinced in the striving for human welfare. If it did not, ethics would begin where Garnett leaves off. It is clear that in Garnett's view "good" *does* include "moral good", and, once more, it is his logical consistency and not his moral insight which fails him. If he had had less moral insight he might, like Bentham, have got away with it.

Acting on the usual academic assumption that clarification of issues is to be preferred to manners, I have concentrated on those aspects of Garnett's thesis which I find difficult. I must make brief amends by indicating the countervailing excellences. His account of "conscience" is central, and it is admirable. Some word is needed for the moral judgment which pronounces in particular cases on the *moral* suitability of the various possible courses of action, and in appropriating for the purpose the word "conscience" he is going behind the reduced and mechanized notion of a response to rules, and reloading the word with its original content. He secures it against fanatics and sceptics by insisting both that it is exploratory and highly fallible, and that it must none the less, after due deliberation, be followed.

¹ "Goodness leaves unlimited scope for special moral values, for values which are by no means materially completed in mere goodness." *Ethics*, II, 190, Eng. Trans.

"Conscience" has had a bad press from recent writers as an example of "intuition"; and what they object to is an alleged assumption of infallibility. As a matter of moral experience, it makes no such pretensions, except when it becomes identified with rules. Garnett correctly notes both the fact and the exception. His account of free-will as "effort", and the situating of that effort at the moment when the mental horizon can be contracted or enlarged, and the dissociating of it from the moment of overt action, is psychologically sound, and his defence of it ethically indispensable.

We now pass to the account of religion.

It is clear from the start that Garnett's sympathies in the matter of religion are limited. He has little feeling for ecclesiastical tradition, not much more for mysticism, a vehement dislike of Protestant neo-orthodoxy, an indifference to Catholic demonstrations of the existence of God, together with a fear of their moral consequences, an excessive distrust of theology, and an insufficient awareness (despite his Australian background) that to be a religious conservative in the American Middle-West is to be a naïve radical elsewhere. But within his limits he is sincere, impressive, and profound. The limits, of course, are those of an Anglo-Saxon moralist. It is clear that his personal religion, which seeps through into the argument and lends so much of its weight to it, springs from a moral root, and would not have sprung from any other root. This peculiarity would be a disqualification if he were proposing to write on religion *per se*. It is almost an advantage in writing on Religion and the Moral Life.

"Almost", and not "conclusively". For while his special emphasis lends him unusual insight into the front which religion and morality occupy in common, it also leads him to assume that this is the whole of the front on which religion is engaged. Now this assumption is closely linked to his unilateral view of the moral-religious relation. If the moral foundations of religion are the only foundations which count, *of course* religion will give back nothing but what morality has already put into it. But no one but a very moral Anglo-Saxon could possibly think thus about religion. Every religion worth its keep has been morally subversive, and Christianity most of all. To start with, there are the specific so-called "theological" virtues of humility and charity which derive directly from an awareness of the presence of God, and, moreover, cannot be placed in a separate compartment, for they profoundly affect the operation of the "natural" virtues. They take the arrogance out of them and make them fit

to live with. It is true, as Garnett asserts, that to practice the "natural" virtues it is not necessary to worship; but those who do worship practice them with a difference. Religion is far more influential in the formation and guidance of morals than Garnett allows: and it is certainly not the case, as he all too frequently suggests, that the main function of religion is to provide power for an ethical order which it does not alter. The moral point of view has to be transcended, or morality will become a form of idolatry—it is *the* typical Anglo-Saxon form of idolatry. As Bosanquet remarked,¹ the moral view cannot be the final view: for who could approve Falstaff, yet who would willingly consign him to hell? The fact is that the "whole man" on whom Garnett rightly lays such stress is not the "moral" man, but the man whose whole experience, including his intellectual experiences, is gathered up and sustained and transmuted in a standing attitude of worship—in fact, the religious man.

The same point comes out in his treatment of sin. There is nothing in his discussion (R.M.L., p. 201-2) which suggests that sin is anything other than immorality. His conviction that the term is in fact not really needed is given away when, in the index, he places it in inverted commas (R.M.L., p. 222). But religion simply cannot do without it. When Peter exclaimed, in an awe verging on terror, "Depart from me, for I am a sinful man, O Lord", what he said does not receive cash value in: "I ought not to have charged the village idiot so much for that fish last week", or in any number of such remorseful reminiscences. The sense of sin, in fact, is the sense of distance from God, of which overcharging for fish may indeed be an illustration, but which may equally well be illustrated in the highly scrupulous conduct of a Pharisee. Anyone who down-grades sin into immorality misses a whole dimension.

It is far from Garnett's intention to denigrate religion, and he is particularly emphatic that religion is an ultimate devotion to something other than oneself: "Man's religious need is not of a God to serve him, but of a God to serve" (R.M.L., p. 91). On this he has many timely and important things to say, and we shall shortly elaborate them. All that is here contended is that they do not fit in with his insistence on the "independence of ethics". It is not enough to admit, as he does freely enough, that the religious perspective *outruns* the ethical. It has to be allowed that it may *change* it. A religion which was merely a confirmation and a prolonging of the ethical witness would be constricted, at its most sensitive point, by secular presuppositions.

¹ *Principle of Individuality and Value*, p. 17.

Turning to Garnett's account of religion itself, there is much for the philosopher to admire. The initial problem of faith and reason (as M. Gilson insists, the starting-point of mediaeval philosophy, and inevitably the starting-point of any philosopher who is at all interested in the relation of his philosophy to his life) he faces squarely and, what is more, seriously tries to solve. On the one hand, he does not believe that faith is any kind of certainty. All faith, he says, has an element of risk about it; and he even goes so far as to add that if there were certainty there would be no occasion for faith. It is significant that it is the moral argument against having certainty that appeals to him: he does not discuss the ontological or cosmological proofs on their merits, contenting himself with urging that if they were true, one of the finest traits of human nature would become obsolete. The matter cannot be left there: the proofs *must* be discussed on their merits, and it could be seriously debated whether certainty *does* put an end to the need for faith: in one sense of the word at least, a man can have certainty and lack faith. On the other hand, Garnett holds that the leap of faith should be taken, not out of absurdity or in the void, but from the vantage point of a reasonable probability. Faith, in fact, always requires evidence and it always goes beyond the evidence. To indicate the role of faith in the life and mind of a believer, this description could hardly be bettered.

Though he sees a certain probability in the argument for design, it is in the moral life that Garnett finds the best of all evidence for the "faith of Theism": and the fact which above all impresses him is that our moral duties and, for that matter, our moral affections, present themselves to us as authoritative and inescapable—and none the less so when, instead of being read off from rules, they have to be discovered from case to case. We know perfectly well what it is to conform to social pressures and to set up goals for ourselves, and the moral imperative, whatever it is, is neither of these things. In saying that it comes from the "Source of our being" we are going beyond the evidence, but in saying that it comes from society or ourselves we are going against the evidence. Its presence in the life of man points to his belonging to a larger life.

This line of argument, which, as Garnett points out, stems back to Kant and has been taken up in our times by Professor John Baillie, is one which strongly moral natures will always find congenial, and it is not surprising that it should turn up in the Middle West as well as in Königsberg and Scotland. Garnett would, I am sure, admit that there are others, adapted to those

whose experience of God is not so unilaterally moralistic, and that the most impressive feature of the whole situation is the convergence in the centre of extremely different interests and temperaments. But, in this as in the other approaches, there is one point to be emphasized. Every evidence of God in the world is clouded by imperfection. The moral case for God is not merely that the moral life reflects Him, but that it reflects Him obscurely. Here, as in other human situations, God is menacingly distant as well as in comforting proximity, and the distance is what drives us to look beyond our human condition. It would be wrong to say that Garnett ignores the distance, but he certainly plays up the proximity. The advance from morals to religion he conceives as progress in a straight line, free from dialectical stress and frustration. He is in fact a traditional American "once-born" of more than usual depth and understanding. The rational thing for the "twice-born" to do is to salute him as "the other half of the orange".

It is inevitable that a reviewer should see books from an angle other than the author's, and it is right that he should conclude by explaining why. It would not be gathered from this review that much of Garnett's polemic is directed against "humanism". He will undoubtedly expect to be attacked from the left, and it may dismay him to be attacked from the right. The reason is that the reviewer agrees entirely with the polemic against the "humanists", and thinks that too many concessions have been made in the effort to get on terms with them. But, both in his polemic and in his analysis, the author says far too many good things to be allowed to suppose that the reactions he arouses are mainly critical. He has, in fact, given us in these two volumes a sincere, well-argued and far-reaching essay in religious humanism; and at a time when both religious inhumanism and irreligious humanism are rampant, and there is so much good in each of them, along with bitterness and exaggeration, his reconciliation of values, even if somewhat too easily achieved, is instructive and encouraging. Personally, I can take more Barth, less Fromm, and no Dewey; I am right-centre to Garnett's left-centre; but as this is a case in which the centre parties have more in common than either has with its own extremists, I should not wish my differences from him to blur appreciation of a sustained and admirable enterprise; and I am sure that what he has written will remove much culpable misunderstanding.

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THE LOGIC OF MORAL DISCOURSE. By Paul Edwards. Glencoe, Ill., The Free Press, 1955. 248 p. \$4.00.

Dr. Edwards is lively, clear and uninhibited. No notions of academic decorum prevent him from lashing out at the people he dislikes, whether they are intuitionists ("Intuitionism and, more generally, all forms of non-naturalism in ethics from Plato to Ross, have fundamentally had one and only one purpose: to help support the morality of self-denial and sin . . . the moralities of the fuddy-duddies and the sour-pusses."—p. 240) or his Australian landlady ("Z.Z. is a member of the League against Vice and Moral Corruption She has a little boy whom she brings up in strict discipline, 'cleanliness' and fear of God. Her cruelty comes out in a thousand ways. Even Australian judges, the most brutal outside the Soviet Union, are too lenient for her taste. There is not enough corporal punishment either in prisons or in the schools Every serious question her boy asks she answers with a lie or an evasion. She is in the forefront of every movement to ban a book or movie. She contributes regularly to religious causes and is one of the pillars of her congregation. Her chastity is unrivalled."—p. 145).

This is at least fresher than the moral problems about borrowed books and promises made to dying creditors which too often seem to bound the horizons of moral philosophers. For Edwards is not merely a rhetorical defender of "the new morality" of about 1920: he is, as his title suggests, a serious student, in the modern manner, of "metaethics". It will be seen that, unlike some of his fellows, he does not shrink from drawing conclusions about the desirability or otherwise of actual ways of behaving. Nor does he ignore or decry the traditional questions with which moral philosophers, "from Plato to Ross" and beyond, have concerned themselves. Indeed, the dust-jacket tells us that "the first four chapters are in effect a general introduction to ethics, presenting the main theories in some detail with statements of all the standard arguments for and against each theory". This is, however, too large a claim. Edwards' account of the "standard arguments" is too brief and too superficial to be a safe guide for the beginner. He can hardly, in this space, consider the standard answers to the points that he makes. What these chapters really do is to put Edwards' own theory in perspective. If they are not entirely successful, it is because Edwards himself sometimes seems to miss the real point of the theories he discusses.

What is Edwards' own theory? On the dust-jacket it is described as "a detailed restatement and defense of the emotive

theory". From the body of the book, however, it seems doubtful if he would altogether agree with this. What he actually says is: "I do not know of any naturalistic theory, from Hobbes to Stevenson, which has successfully withstood the critical fire of intuitionists or of rival naturalists Now, some forms of naturalism, like naïve subjectivism, have been altogether mistaken about these relations [i.e. the relations of ethical predicates to attitudes and emotions, and to "the natural features of the subject of the moral judgment"]. But other theories, such as, e.g., the theories of Ayer and Stevenson or those of Dewey and Mill, while they succeeded in bringing out some of these relations, failed to bring out others that are equally important." (p. 141). Again, one feels inclined to say simply that Edwards is defending subjectivism; but he himself says, on p. 47, that he "rejects all forms of subjectivism" I must explain what he means by this.

Two of the four introductory chapters deal with subjectivism. Here Edwards makes (though he does not of course originate) a useful distinction between what he calls "naïve subjectivism" and what he calls "the error theory". It is quite obvious that many of the traditional arguments against subjectivism, such as most of those advanced by Moore or Ross, prove only that a man who says "X is good" *thinks* he is saying something about X and not merely about his own emotions. It might be said (though this is not quite the point that Edwards makes) that it is misleading to talk about the *meaning* of "good" here; because there is a sense in which a man must mean what he thinks he means. But it is not in this sense that the subjectivist contends that "good" means "approved" (or whatever). It would be much more accurate to say that, according to the subjectivist, "good" *really means* "approved". Or, as Edwards expounds the theory: "Moral judgments refer to something outside the speaker's mind but are always false, since all that the speaker has a right to say is that he has a certain feeling or attitude". It is this form of subjectivism that he calls "the error theory". This is, it seems to me, the only form in which subjectivism has been seriously defended: the "naïve subjectivist", who has drawn the fire of most of the critics, is a man of straw. Edwards points out that in ordinary speech the statement that something is right may sometimes provoke the reply "You mean that *you* are for it"; and sometimes, "All that you are entitled to assert is that *you* approve of it". Naïve subjectivism, he says, is an explicit formulation of the metaethic implicit in the first of these remarks; the error theory an explicit formulation of the metaethic implicit in the second. But I should have thought that in ordinary speech

the two remarks are, in fact, equivalent: the second is merely a more accurate statement of what is intended by the first. "You mean" in the first remark means "you really mean".

In rejecting (as he claims) all forms of subjectivism, Edwards rejects the error theory. His discussion of it centres round Mackie's article "The Refutation of Morals" (this *Journal*, 1946), which, he says is the "fullest exposition" the theory has received. It is a mistake, Edwards says, to suppose that a statement like "That lecture was boring" refers to a non-natural quality of "boringness" which the speaker imagines the lecture to possess. Nor does it refer to the speaker's "attitude", his boredom. The remark *expresses* his boredom; but it *refers* to certain objective, natural features of the lecture: its length, its unintelligibility, and so on. This gives us the clue to Edwards' moral theory; though it takes a good many more pages, filled with lively (if slightly irritating) discussions of such remarks as "the steak at Barney's is rather nice" before we get it stated plainly. What it amounts to is this: If a member of the League against Vice and Corruption says that Mrs. Z.Z. is a good woman, he does not, as the naïve subjectivist would contend, say something true about his own attitudes. Nor does he, as believers in the error theory contend, say something false about Mrs. Z.Z. (that she has a non-natural quality, goodness). He says something true about Mrs. Z.Z. (that she has the natural qualities, or some of them, which Edwards himself enumerates in the character sketch quoted above). If Edwards says that Mrs. Z.Z. is a bad woman, he is also saying that she has those qualities, or some of them. The two statements are objectively different only in so far as they do not refer to the same qualities; and it is conceivable that Edwards and the League member might have precisely the same qualities in mind. In that case the two statements differ only in what they express: a favourable attitude in the League member, and an unfavourable attitude in Edwards. To that extent the emotivists are right. But they are wrong if they suppose that moral statements cannot be true or false. They are wrong, too, if they suppose them incapable of rational justification, or if they say that moral differences cannot be settled by argument.

Here Edwards invokes the now familiar parallel with induction. There is a sense, he says, in which our belief that the sun will rise to-morrow morning is justified by the fact that it has risen on all the other mornings. That fact is "a good reason" for that belief, in a perfectly good sense of the expression, though not in the logician's sense. Similarly, Edwards seems to think, if

one is asked to justify the statement that someone is a good woman, one can do so quite "rationally" by pointing out that she is kind and gentle. But it is obvious that the parallel breaks down once it is realized that what Edwards thinks a good reason for calling Mrs. Z.Z. a bad woman is, for the League member, a good reason for calling her a good woman. Nothing like this happens with induction. The difficulty can be evaded for a time by introducing the distinction Edwards makes between first-order and second-order reasons. The League member might say that Mrs. Z.Z. was a good woman because she prevented her child from reading "immoral" books: this would be a first-order reason, and Edwards might give precisely the same reason for calling her a bad woman. But if asked why he thought this a good reason, the League member might say that her behaviour showed that Mrs. Z.Z. wished to save her child and his associates the suffering that comes from immoral behaviour; while Edwards would (it is fairly clear) say that she was merely giving vent to her sadistic tendencies, and that she had no real knowledge of the effects of reading the books she condemned. The disagreement now turns at least partly on questions of fact (the effects of censorship and the motives behind it) which can be settled.

This is of course well-trodden ground; and it has often been pointed out that it is at least doubtful whether all moral disagreement can be reduced to disagreement about matters of fact. In the end Edwards admits this: there are, he says, "fundamental moral judgments" for which no reason can be given. These, he declares, have "emotive meaning" only. He seems to think, however, that this admission is of relatively small importance.

"What I have been saying in this section may sound like a retraction of the objectivism which I have been advocating throughout this volume. Such an impression would only be very partially correct. Of course, if by 'objectivism' one understands a metaethic which maintains that all moral judgments without exception have objective descriptive meaning, then I am not an objectivist. To this observation it should be added, however, that if I am not mistaken, fundamental moral judgments are very rare in actual discourse. Especially when people engage in moral disputes, the conflicting moral judgments are practically always non-fundamental." (p. 190). And on the next page Edwards speaks of "such artifacts as 'Happiness is an intrinsically good thing'—myths concocted by philosophers who have temporarily lost all contact with ordinary human discourse".

Now this is to miss the whole point. Analysis of "ordinary human discourse" (including Edwards' own analysis) shows, if

it is carried far enough, that all moral judgments without exception depend (logically) on "fundamental moral judgments", whether or not the speaker ever formulates these to himself. To see this is not "to lose contact with ordinary human discourse" any more than Edwards does when he asserts that moral predicates refer to natural features of the subject to which they are applied; for it is obvious that it may take some deep dredging to discover what these features are. Edwards tells us that when persons of "average Western taste" say "the steak is rather nice" they mean, among other things, that it is tender, made of very fresh meat and fairly thick, but that when Indians say it they mean that it is peppery, tough, thin and burnt. Many members of either group, however, would be quite unable to specify these characteristics if asked to explain themselves. And certainly ordinary speakers would be puzzled by Edwards' assertion that "Mrs. Z.Z. is a good woman" (said by the League member) and "Mrs. Z.Z. is a bad woman" (said by him) have the same objective reference. To point this out is not in the least to refute Edwards; and similarly he has said nothing to refute those who say that ultimately moral judgments imply some such belief as that "happiness is an intrinsically good thing", even though this belief may not be explicitly formulated.

His failure to see this makes his whole discussion of the traditional moral theories unsatisfactory. For it is in fact "fundamental moral judgments" that subjectivists and objectivists have been disputing about. No one would deny that questions about the most efficient means to an agreed end are purely objective ones, which can be settled by reasoned discussion: the problems begin when assertions are made about the desirability of the ends themselves. And, when such assertions are not made, they are always implied: so that "fundamental moral judgments" really are of fundamental importance. This is the familiar point about the impossibility of deriving "ought" from "is", or about the dependence of hypothetical imperatives on categorical ones. But Edwards says blandly: "In a perfectly familiar sense of 'categorical' there are millions of imperatives which are categorical and perfectly meaningful"; to admit this, he thinks, is not in the least to "surrender to intuitionism". One example he gives is a doctor's injunction: "Drink a glass of milk every night before going to bed!" Edwards counts this as "categorical" because the doctor does not add in so many words "if you want to put on weight", though he admits that this is taken for granted. This can hardly be the crass ignorance it seems: one can only suppose that Edwards is being wilfully obtuse about what

philosophers have meant by "categorical imperative". (And nobody else uses the expression.) Yet he uses this to support the assertion that "categorical as well as hypothetical imperatives . . . follow from or are implied by certain factual statements" (p. 133). This is the merest quibbling.

If it is true that the dispute between subjectivists and objectivists has been about fundamental moral judgments, then it is clear that Edwards has not, as he claims, taken account of those features of moral judgments to which objectivists draw attention: he has ignored the main objectivist arguments altogether. Nor has he rejected all forms of subjectivism: he is on the contrary defending one form of it. The test of his theory is clearly his ability to reconcile it with his obvious conviction that his own fundamental moral judgments are somehow superior to Mrs. Z.Z.'s. Because of his belittling of fundamental moral judgments, Edwards does not give this question the attention it deserves; but he touches on it in the three or four pages which deal with "Higher and Lower Moralities" and "Moral Blindness" (Edwards' sub-headings).

"I believe that the morality, i.e., the moral recommendations contained in Reich's *Sexual Revolution* and Russell's *Marriage and Morals* is higher than the teachings of the Catholic Church or Lord Elton or Mr. Sokolsky on sexual questions. What I mean by calling the former morality higher is (roughly) that it is based . . . on extensive clinical and sociological data of which the other side is totally unaware, and that, unlike the other morality, it is not the result of neurotic anxieties and resentments and of superstitious errors. This, too, is clearly an objective claim and . . . one which is true." (p. 213.)

Now the first of these criteria cannot apply to fundamental moral judgments, since it is part of Edwards' definition of these that they are not based on *any* data. I doubt if the second criterion is meant to apply to them either, but perhaps it could. Neurotic anxiety is either anxiety about something which it is reasonable to dread, but which is not likely to happen, or anxiety about something that is not worth worrying about. Only the second of these could be called a neurotic fundamental judgment. But if we do call it that, we imply that it is false: that some things (pain, poverty, disgrace, causing pain to others) really are worth worrying about in some ultimate sense (are bad in themselves), while some other things (one's own sinfulness, lack of purity, etc.) are not. Of course it may be that the neurotic worries about these things only because he attributes some further characteristic to them which they have not got; but then it is not

his fundamental moral judgment (that whatever has that characteristic is worth worrying about) that is neurotic. It is clear that, so far as the term "neurotic" does apply to fundamental moral judgments, it is not, in Edwards' sense, "an objective claim" after all; that is to say, it is not one that can be empirically tested.

Much the same point can be made about Edwards' treatment of "moral blindness". The phrase, he says, normally refers either to "cases where a person acknowledges a certain general moral principle but then arbitrarily fails to apply it to a certain group or a certain person" or to "people who are devoid of sympathy" (p. 215). On the simple point of usage, I have doubts about the first of these assertions. Suppose a woman believes firmly that unmarried mothers should be punished severely, but in fact treats with great kindness the only unmarried mother she happens to know. Those who agree with her on the general principle might call her "morally blind"; others would be more inclined to say that she was less morally blind than they had thought. On the other hand, someone who did apply this principle quite rigorously (to her own daughter, say) might well be called morally blind. Inconsistency in applying a principle is not called moral blindness unless we approve of the principle; and if we disapprove of the principle, consistency in applying it may equally be called moral blindness.

Edwards would no doubt say that here we have the other sense of "moral blindness": lack of sympathy. "What we mean in these cases is, I think, simply that the person is devoid of or *deficient in sympathy* for certain other living creatures. Now, if anybody wants to call 'sympathy' a special faculty or even a moral faculty, I would not enter a protest. But sympathy is certainly not identical with the moral intuition whose existence the intuitionists assert. It is something which no sensible naturalist denies or needs to deny." (p. 216; Edwards' italics.)

Now it may be true that "morally blind" means "deficient in sympathy", but this in itself means more than "without sympathy". We say: "without fear", "without false pride", and equally "without scruple", "without compunction"; but to call someone "deficient in" a quality is to imply that he lacks something he ought to have, and not merely something that he might have. The "faculty" that the intuitionist talks about is, of course, not sympathy but the power of judging which qualities a man ought to have, and which ones he would be better off without. And Edwards has said nothing to dispose of the claim that words like "deficiency" do presuppose some such faculty. And it is of course just this that is implied, with even more force, by the

term "morally blind": that the unsympathetic man has failed to see something, and that what he has failed to see is not just that people are suffering, but that their suffering should be relieved. In short, if Edwards' account of this usage is correct, then there is both a utilitarian and an intuitionist ethic embodied in it. Of course this does not mean that either intuitionism or utilitarianism is sound: the implications of the term "morally blind" may be misleading. Edwards is quite entitled to say that the cash value of the term, or what it "really means", is simply "without sympathy". What he is not entitled to say is that those without sympathy are deficient, and certainly not that they are "morally blind" in the full, traditional sense. More generally, he is not entitled to say that his ethical theory permits him to speak of "moral blindness" in that full sense. Yet I think that he does want to say this.

Edwards' reluctance to admit the full implications of his own theory comes out in the final chapter, in his discussion of the charge that "if the theory were true then neither party in a moral dispute could ever prove his case, then nobody could ever produce a reason or rational justification for a moral judgment". He says that this charge has "some measure of truth", but "is largely false; and . . . insofar as it is true, it is hardly a charge". (p. 229; his italics.) He makes two points here: that the charge applies only to fundamental moral judgments, and that, when it comes to these, no other ethical theory can prove them either. But this is to mistake the nature of the charge. It is not so much that Edwards cannot prove his fundamental moral judgments, and so must simply assume them to be true; it is rather that, according to him, they are not properly judgments at all, since they are not the sort of thing that could be either true or false. Nor will it help to say that the charge applies only to fundamental moral judgments, and that disagreement about these is relatively rare. The point is that all moral judgments depend on fundamental ones, and that, if fundamental moral judgments cannot be true, it does not matter whether secondary ones are true or not.

This book will disappoint those who (like the present reviewer) share Edwards' dislike of intuitionism and non-naturalism in ethics, believe that his *fundamental* moral judgments are (on the whole) sounder than Mrs. Z.Z.'s, and would like to feel that in preferring his to hers they are acting rationally.

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D. H. MONRO.

REVIEWS

PHILOSOPHICAL ESSAYS. By A. J. Ayer. London, Macmillan, 1954. 284 p. 18s. (U.K.).

These essays first appeared in various publications between 1945 and 1953. They exemplify that mixture of philosophical approaches—the logical positivist, the ordinary language, and the plain philosophical—which enables Ayer to be offside with everyone at once. But all of them are very good. The style too is remarkable: its even surface, economy and clearness, combined with an avoidance of all digressions, result in the *fastest* philosophising one is likely ever to read.

Of the twelve essays, four are logical or metaphysical (individuals, the identity of indiscernibles, negation, and "what there is"); five are epistemological (sense-data, phenomenalism, basic propositions, statements about the past and about other minds); and three are moral (the analysis of moral judgments, freedom and necessity, and Bentham's utilitarianism). For this review I have simply selected a couple of major points which happen to interest me.

In the first essay Ayer argues that individuals are completely reducible to "their" predicates and that the uniqueness of individuals consists in a predicate or predicates being uniquely instantiated. This makes the Identity of Indiscernibles necessarily true. In the second essay, however, he concedes that it does seem possible to construct counter-examples, that sentences about indiscernible individuals need not be manifestly self-contradictory. At the end he inclines once more to the former position because he "still cannot see how asserting that an individual exists can be to assert anything more than that some predicate, or set of predicates, is instantiated" (p. 35).

I agree that the apparent counter-examples cannot be allowed to outweigh the general argument on the other side. What permits most recent writers to regard the Identity of Indiscernibles as at best contingently true is their belief in two *independent* sorts of identity and difference, viz. numerical and generic. (Some problems this belief gives rise to when numerical identity and difference are equated with spatio-temporal identity and difference are indicated in my article in this *Journal* no. 3 of 1955.) Apparently Ayer does not share this belief, and his

reason for not doing so is just that it would permit indiscernible individuals and that these must be "unknown somewhats". Certainly, if we take a relational view of space and time, as nearly every philosopher at least says he does, then no reference to space or time can specify an individual uniquely, because these relations are repeatable, i.e. are simply further predicates. If on the other hand we take an absolute view of space and time, then it seems that Ayer is right, that our individual places and times are no less unspeakably blank or metaphysically hidden than Locke's material substances. And it is this idea of substance that Ayer won't have (v. p. 31).

Even if "existence" as a substratum of predicates won't do, Ayer's suggestion—"existence" as the instantiation of predicates—seems to me no better. Presumably to say that a certain predicate "P" is instantiated is to do more than merely mention P. What more? If "instantiated" is not a predicate, how can instantiation avoid the charge of being no less a "metaphysical invention" (p. 31) than substance is? But if it is a predicate or is reducible to predicates, what use is it to us for the purposes of making an existence-claim, as we do, e.g., in referring to individuals? It cannot be an accident that Ayer has left it so unclear what statements in his subject-less language would be like. What happens in fact is that the whole familiar world of individuals, which gives its primary sense to talk about existence, has evaporated entirely and we are left with only a free-floating mass of pure predicates.

In other words, we cannot in this language refer to individuals, or in general make an existence claim, at all. This is the prodigious price at which the analyticity of the Identity of Indiscernibles has been bought. For if individuals cannot be referred to in a language consisting entirely of predicates, then it is *a fortiori* certain that indiscernible individuals cannot be referred to.

Neither, of course, can discernible individuals be referred to. In particular, how would we distinguish between one predicate's being uniquely and another's being not uniquely instantiated? Every predicate is intrinsically one predicate, so just how are we to refer to the plural instantiation of some of them? Ayer would say, by reference to the different predicates with which they are co-instantiated. But it would be a contradiction to say, e.g., that P is co-instantiated with R and S and with non-R and S, unless we could add that these are different instances of P; and how can we add that if the instances are to be analysed entirely into such predicates? This means that the predicate itself emerges,

so to speak, as its only instantiation. But once repeatability, the possibility of plural instantiation, is ruled out, what right have we to speak any longer of predicates? This can be put more simply as a direct argument against the Identity of Indiscernibles, thus: if the uniqueness of an individual consists in the unique instantiation of some predicate(s), then, since predicates are by definition capable of plural instantiation, it must be at best contingently true, if it is true, that the predicate(s) in question is not instantiated elsewhere.

The theme of individuals and predicates comes into this book surprisingly often. It comes into the essays on the past and on other minds in this way. Ayer thinks that what identifies a statement as being about a *past* time or an *other* mind is not anything in the content of the statement, but just the use of the appropriate tense or personal pronoun. Then an examination of the meaning of such statements involves considering the kind of impossibility there is in the speaker's being in the best position to verify what he says; and this, certainly in the case of minds, and perhaps in the case of times, involves considering the sort of impossibility there is in one individual's having been another. But the theme comes up again very interestingly, and Ayer's view of individuals brings its consequence in here too, in the longest and most systematic of all the essays, "Phenomenalism". Ayer still thinks that the sense-datum language is "logically prior" to the material object language (v. pp. 103-104); and he is at greater pains than ever before to make the former an independent language. Thus in meeting (I think successfully) the objection that phenomenalism requires some actual events to have hypothetical causes, Ayer tells us that in speaking about sense-data not only is the word "cause" inapplicable—this ought to be familiar ground—but the word "event" must not be taken in its physical sense: sense-datum events are, for one thing, in sensory, not physical, time. This is consistent, though already the sense-datum balloon seems to be tugging wildly at its moorings. It flies off irrecoverably, however, when we are told that "there can be no where and when *of* the [sense-datum] story", because "all the wheres and whens are supposed to be included *in* the sensory story". (p. 161. *Italics in text.*) This is still admirably consistent, but could it be made more clear that the sensory stories will not allow us to refer at all to particular material objects as we aim to do when we say "this ash-tray", "the building that used to be here", and so on?

In fact it is now doubly impossible to do so. It is impossible because of the evaporation of individuals into predicates that the

first two essays bring on. And now all those physical predicates in turn are to be analysed in terms of sensory predicates.

Nor is this all. The same process of evaporation entails that we cannot make an existence-claim even about sense-data! For their being "sensory" predicates (if indeed Ayer is entitled to use that adjective) makes no difference here, they are predicates still and that is what counts. So there ought to be no concessions made (as there are in pp. 150-163) to talk about "individual sense-histories", let alone to talk about "travelling" along "sensory routes", and "sense-fields" "between" other "sense-fields". Even when Ayer says, having explained that a full phenomenalist translation of a material-object statement would never mention an observer's body, that "the phenomenalist's tale . . . is, in that respect, a tale that tells itself" (p. 163), the paradoxicality of the position is not quite fully revealed; for the semi-facetious words "story" and "tale" still continue to make us imagine we have not been divested of everything we could call "what exists". Not having seen all the implications, we imagine that we are at least being allowed to retain a world in which individual sense-data succeed one another. But not only does the tale tell itself, and nowhere and nowhen at that; but it can refer to no individual sense-data, and even the succession of such individuals is, like them, to be resolved into (sensory) predicates.

I don't know what Ayer thinks the world is really like, but if he thinks it is like what these essays imply, then the echo of Hegel in the last quotation is appropriate enough. The picture I get is of a timeless, spaceless and individual-less exfoliation of predicates, or rather, of some kind of unity of all predicates among which are included times, spaces, and (what ordinarily pass for) individual subjects. I can't even say that I'm sure this is the wrong way to see the world; at least I know of no other theory of individuals and predicates or of identity and difference which is free from equally strange-sounding consequences. But these two comments ought to be made on Ayer's position. First, the ideas of subject and predicate seem to be so linked that it may be futile to try to make one of them do the work of both. It is a consequence of their inseparability that when we try to put to ourselves a world-picture containing only predicates, the idea of a subject of which these are the predicates irresistibly reasserts itself in the form of some background against which or atmosphere in which they exist. Second, such a world-picture affronts our sense of reality, for what it is worth, enormously, just because it takes away at one stroke that world of particulars which we, like the British Empiricists, think of as containing everything which, in the primary sense, "exists"

After recent "ordinary-language" ethics, I find Ayer's 1949 essay "On the Analysis of Moral Judgements" particularly refreshing. It is essentially a restatement, with many qualifications, of the earlier "emotive" theory. Despite his long explanations of what one is *not* doing when one takes up such a subjectivist stance in meta-ethics—that one is not denigrating morals, or licensing people to do whatever they like, or forbidding oneself to make moral judgements—I am still unclear about just what one *is* doing. There is, it is true, one easy way to explain what such a philosophy of morals means, viz. by simply denying the objectivism of Moore's *Principia* or of Anderson; but this is too easy, just because these writers are so hyper-objectivist as in effect to have forgotten that moral problems are practical (cf. what Ayer says pp. 240-243).

The reason, Ayer says, why his analysis of moral judgements isn't any of the above things—a denigration of morals, or a principle of tolerance, etc.—is that to take it in any of these ways is to base a particular moral policy on a general analysis of the expression of moral policies. The emotive theory is 'meta-ethics', and nothing at all can be deduced from it as to any particular ethical judgement. It can neither condemn nor support any or all moral policies.

But to me it seems that Ayer is here engaged in *recommending* a sharp distinction between meta-ethics and ethics, rather than pointing it out; for unfortunately there just is a kind of stickiness about ethical judgements such that even to frame a theory in meta-ethics is itself to make an ethical judgement. I cannot imagine the emotive theory, e.g., being so phrased as to be free of all suggestion of subverting, if only by tolerating, all moral policies alike. I think this is borne out when we watch what happens when someone teaches a subjectivist meta-ethics in a university. And it's no good saying that even if it were true that subjectivism in meta-ethics has these effects *causally* (Ayer very much doubts this though, cf. pp. 248-9), still a universally-tolerant, or any other, moral policy would not follow *logically* from it. If, as I am suggesting, subjectivism in meta-ethics cannot be stated non-toxically, then what it entails cannot be decided while we ignore the way it is actually taken by intelligent people.

Indeed, how can it be denied that there is a connection which it would be misleading merely to call "psychological" between moral tolerance and an analysis of moral judgements like Ayer's? Putting it another way, everyone would feel the inconsistency of a man who held such a philosophical view of ethics and at the

same time with ferocious strictness lived by and demanded that others live by some particular moral policy. And that is only the extreme case. Ayer tells us that he himself makes moral judgements, and we can surely believe him, since everyone does; but this is only a lesser degree of the same inconsistency. The fact that this is felt to be an inconsistency reveals that the enunciation of this meta-ethical theory is itself taken as the assumption of a moral stand-point.

Nevertheless, I think that an analysis of moral judgements like Ayer's is the most hopeful of all positions in moral philosophy. The fact that one continues, even after accepting such a meta-ethics, to make at least some moral judgements of just the kind and the content one did before, might pass for a mere fact about human nature, the result of training and so on. But it is much harder to get around the more "internal" kind of inconsistency, viz. that to accept such a meta-ethics is itself to make a moral judgement and positively to take stock in ethics while announcing that one does not.

D. C. STOVE.

THE PHILOSOPHY OF THE ENLIGHTENMENT. By Ernst Cassirer. Translated by Fritz C. Koelln and James P. Pettegrove. Princeton University Press, 1951, 366 p., \$6; London, Geoffrey Cumberlege, 40s.; Beacon Press, paper-back ed., 1955, \$1.45.

Like the entire lifework of Ernst Cassirer, the present volume shows an equal mastery in elucidating the fundamental problems of science and of the humanities.

In the panoramic view of the eighteenth century philosophy which is unfolded in this volume, fortunately now translated from the German edition of 1932, Cassirer removes especially two old prejudices against the thought of European enlightenment. He carefully tries to show why the philosophy of that epoch was not one of mere criticism, of destructive analytic dissection or of shallow "reflection", as Hegel had once indicated with the acclaim of numerous other historians. True, the leading thinkers of that century were rather critical of the "esprit de système" of their predecessors which manifested itself most strikingly in the deductive chains of argument of Spinoza's all-embracing system. But D'Alembert, Hume, Diderot and other great representatives of that time certainly did not lack a highly constructive, synthetic "esprit systématique".

Further, Cassirer vigorously defends eighteenth century thought against the frequent reproach of neglecting the colourfulness of facts in favour of far too strong a belief in abstract reason—a belief, it is true, to be found in the æsthetics of Gottsched as well as in the political practice of the French Revolution. Cassirer's work was thus the first of at least three great contemporary attempts at a thorough re-evaluation of the philosophy of the Enlightenment. Karl Joël, too, depicted in his *Wandlungen der Weltanschauung*, vol. II, 1934, how much power of liberating and making concrete lives in the philosophy of the eighteenth century, not only in minds like that of Buffon; and Paul Hazard's three volumes on *La Pensée Européenne au XVIIIe siècle*, 1946, follow quite similar trends.

In contrast to these two later works, which offer a vast array of graphic details, Cassirer's work concentrates on the analysis of far fewer principal ideas, such as those of Maupertuis' and D'Alembert's philosophies of nature, Condillac's, Hume's and Tetens' psychologies and epistemologies, Tindal's and Voltaire's philosophies of religion, Montesquieu's and Herder's philosophies of history, Rousseau's and Condorcet's philosophies of society, Dubos', Lessing's and Burke's æsthetics.

But despite or just because of this self-limitation to rather abstract perspectives, this work will remain of lasting value as long as the definitely secular philosophizing developed in the eighteenth century occupies the modern mind.

As to the English translation of Cassirer's work, it seems to me doubtful whether, for instance, the term "abstract sciences" can pass for an adequate rendition of the German term "Geisteswissenschaften". But this is not of great importance.

DAVID BAUMGARDT.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

BATTAGLIA, Felice. *La valeur dans l'histoire*, tr. by M.-L. Roure. (Philosophes Italiens) Paris, Aubier, 1955. 206 p. 525 fr.

French translation of an Italian book first published in 1948. Discusses the theories of Croce and Gentile.

BERKELEY, George. *Works*, ed. A. A. Luce and T. E. Jessop, Volume 7. London, Nelson, 1955. viii, 389 p. 30s. (U.K.).

Contains the sermons, the *Guardian* essays, travel journals, the proposal for the American college, etc.

BRAITHWAITE, R. B. *An empiricist's view of the nature of religious belief; the ninth Arthur Stanley Eddington memorial lecture*. Cambridge University Press, 1955. v, 35 p. 3s. 6d. (U.K.).

Maintains that religious assertions are merely a way of proclaiming one's intention to follow a particular moral policy. "A man is not, I think, a professing Christian unless he both proposes to live according to Christian moral principles and associates his intention with thinking of Christian stories; but he need not believe that the empirical propositions presented by the stories correspond to empirical fact."

BRAITHWAITE, R. B. *Theory of games as a tool for the moral philosopher; an inaugural lecture delivered in Cambridge on 2 December, 1954*. Cambridge University Press, 1955. 74 p. 6s. (U.K.).

A mathematical technique for estimating the fairness of a proposed compromise between conflicting aims.

BRIDGMAN, P. W. *Reflections of a physicist*. 2nd ed. New York, Philosophical Library, 1955. xiv, 576 p. \$6.

A collection of Bridgman's writings on "operationism" since he published *The Logic of Modern Physics* in 1926. There are ten new papers, comprising about 160 pages, in this edition.

CHAPMAN, Dennis. *The home and social status*. (International Library of Sociology and Social Reconstruction.) London, Routledge & Kegan Paul, 1955. xvii, 301 p. 35s. (U.K.).

Investigates the houses, furniture and attitudes to their homes of Liverpool families from different social classes.

COLE, G. D. H. *Studies in class structure*. (International Library of Sociology and Social Reconstruction.) London, Routledge & Kegan Paul, 1955. vii, 195 p. 21s. (U.K.).

Six essays, including statistical and historical material, on such topics as the influence of technological changes on class structure, the concept of "the middle classes" and the presence of *élites* in British society.

DASGUPTA, Surendranath. A history of Indian philosophy: volume V, Southern schools of Saivism. Cambridge University Press, 1955. xii, 204 p. 25s. (U.K.).

EPICETUS. The philosophy of Epictetus, ed. by John Bonforte. New York, Philosophical Library, 1955. xiv, 146 p. \$3.

A re-arrangement of the text of T. W. Higginson's translation, with minor alterations. "I know not a single word of Greek. My only excuse for offering this version of the Discourses is to give the present day, hurried reader an opportunity of reading the philosophy of Epictetus with a minimum of effort." Preface.

FLEW, Antony, and MACINTYRE, Alasdair, eds. New essays in philosophical theology. London, S.C.M. Press, 1955. xii, 274 p. 21s. (U.K.).

A collection of articles on the philosophy of religion, from varying viewpoints. Two were first published in this *Journal*; others in *Mind*, *Hibbert Journal* and elsewhere. Authors are: A. N. Prior, J. J. C. Smart, J. N. Findlay, G. E. Hughes, C. B. Martin, A. Flew, R. M. Hare, A. Macintyre, T. McPherson, D. M. MacKinnon, P. Nowell-Smith, A. C. A. Rainer, B. Mitchell, I. M. Crombie, B. Williams, and R. W. Hepburn.

Ideas; a students' quarterly philosophical journal; volume 5, no. 1. Yale University. Department of Philosophy. 46 p. 35c. (\$1 p.a.).

Edited by graduate students. This number contains articles on the philosophy of religion, meaning, sense data, logical fallacies, and existence. "We definitely need more articles from sources outside of Yale . . . *Ideas* is a philosophical journal prepared by students, and is open to contributions from anyone provided that he be a student." Introduction.

Instituto de Filosofia, Argentina. Symposium sobre existencialismo. Rosario, Universidad Nacional del Litoral, 1955. 208 p. Price not given.

Seven paper-covered booklets on existentialism, each by a different author. The topics dealt with include existentialism and language, existentialism and literature, existentialism and psycho-analysis, existentialism and medicine, Kierkegaard, Husserl, and the treatment of feminine psychology in existentialist plays and novels.

JALABERT, Jacques. L'un et le multiple; de la critique à l'ontologie. (Université de Grenoble Publications de la Faculté des Lettres.) 166 p. Paris, Presses Universitaires de France, 1955. Price not given.

"L'Un est la marque de l'Etre; le Multiple pur serait le Néant . . . Celui qui refuse l'impératif de l'unité, s'éloigne de Dieu et s'éloigne de l'Etre. Il se condamne lui-même."

KAPP, Reginald O. Facts and faith; the dual nature of reality. (University of Durham. Riddell Memorial Lectures, 27th series.) Oxford, University Press, 1955. 63 p. 5s. (U.K.).

Argues that modern physics points away from materialism towards "the belief that the whole of reality is composed of two parts, one named matter and the other diathetes" (a collective term for various non-material influences). "Some events are random and others ordered. Matter is, by its nature, incapable of creating order. So only the random events can be attributed to the unaided action of matter on matter."

KÖRNER, S. *Conceptual thinking; a logical inquiry*. Published for the University of Bristol at the Cambridge University Press, 1955. viii, 301 p. 30s. (U.K.).

An examination of types of concept, their relation to "the given", and the uses, including ethical and æsthetical, to which they are put. In the course of this the author attempts to illuminate many of the traditional problems of philosophy.

MANNHEIM, Hermann. *Group problems in crime and punishment, and other studies in criminology and criminal law*. (International Library of Sociology and Social Reconstruction.) London, Routledge & Kegan Paul Ltd., 1955. x, 309 p. 28s. (U.K.).

A collection of papers, lectures, reports, etc., on such topics as collective responsibility and punishment, war and crime, capital punishment, juvenile delinquency, etc.

MARTIN, Gottfried. *Kant's metaphysics and theory of science*, tr. from the German by P. G. Lucas. Manchester, University Press, 1955. viii, 218 p. 21s. (U.K.).

"*The Critique of Pure Reason* is a sea that is fed by two great streams: one stream is the new natural science, the other stream is the old ontology . . . The aim of our interpretation is to show the inner connection between ontology and theory of science." Preface. Discusses the influence on Kant of Leibniz and Newton, and criticises the interpretations of some of the German neo-Kantians. Scholarly exposition rather than criticism.

MAURIAC, François. *Words of faith*, tr. by E. H. Flannery. New York, Philosophical Library, 1955. 118 p. \$2.75.

Six papers given to various religious gatherings.

NEW YORK CITY. *Children need families; a second look at delinquency prevention*. New York, City of New York, 1955. 60 p. Price not given.

PRIOR, A. N. *Formal logic*. Oxford, Clarendon Press, 1955. ix, 329 p. £2 19s. 3d. (Australian).

Part I deals with the propositional calculus and the theory of quantification; part II with traditional formal logic, treated from the point of view of symbolic logic; part III with "modal, three-valued and extensional systems".

SAUNDERS, Jason Lewis. *Justus Lipsius; the philosophy of Renaissance Stoicism*. New York, Liberal Arts Press, 1955. xviii, 228 p. \$4.50.

Exposition of the work of a 17th century Flemish humanist who endeavoured to reconcile Christianity with Stoicism. Contains a good deal of information about both the ethical and the physical theories of the Stoics.

TRYON, Robert C. *Identification of social area by cluster analysis; a general method with an application to the San Francisco Bay area*. (University of California Publications in Psychology, volume 8, no. 1, pp. 1-100.) Berkeley, University of California Press, 1955. viii, 99 p. \$1.50.

VALPOLA, Veli. Ein System der negationslosen Logik mit ausschliesslich realiserbaren Prädikaten. (Acta Philosophica Fennica, Fasc. IX.) Helsinki, Societas Philosophica, 1955. 247 p. Fmk. 500.

VAN STEENBERGHEN, Fernand. The philosophical movement in the thirteenth century; lectures given under the auspices of the Department of Scholastic Philosophy, The Queen's University, Belfast. London, Nelson, 1955. ix, 115 p. 15s. (U.K.).

"In the first lecture we shall . . . show how, about 1940, the historians of medieval philosophy had arrived at a generally accepted interpretation of the thought of the thirteenth century. In the later lectures, we can discuss this interpretation and correct or complete it on certain points." Introduction.

WEDBERG, Anders. Plato's philosophy of mathematics. Stockholm, Almqvist & Wiksell, 1955. 154 p. Sw. Crs. 19.

Attempts to construct and elaborate a coherent theory of mathematics from the scattered references in the dialogues and from Aristotle's comments on Plato.

WESTMANN, H., ed. Man in his relationships. London, Routledge & Kegan Paul, 1955. viii, 140 p. 14s. (U.K.).

Papers given at the tenth Present Question Conference held at Oxford in 1954. The speakers included physicists, theologians, philosophers and economists, and their subjects varied from Buddhism to "science and poetic insight" and "man and the machine"

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WYETH, E. R. Education in Queensland; a history of education in Queensland and in the Moreton Bay district of New South Wales. (A.C.E.R. Research Series, no. 67.) Melbourne, Australian Council for Educational Research, 1955. x, 214 p. 28s. (Australian).

NOTES AND NEWS

THE AUSTRALASIAN ASSOCIATION OF PSYCHOLOGY AND PHILOSOPHY.

Annual Congress, 1956

The Annual Congress for 1956 will be held at Canberra from August 17th to August 21st. The first session (the Presidential Address) will take place at 8 p.m. on Friday, August 17th.

The Annual General Meeting of the Association will be held at 7.30 p.m. on Sunday, August 19th.

The Secretary of the Congress is Mr. B. Benjamin, Philosophy Department, Canberra University College.

THE AUSTRALASIAN JOURNAL OF PHILOSOPHY

VOL. 34

AUGUST, 1956

No. 2

MEANING AND GRAMMAR

By MICHAEL SHORTER

There are certain forms of words that some philosophers consider to be meaningful while others hold that they are without sense. Examples of such forms of words would be 'virtue is blue', 'you cannot strike a blow a blow', 'I met the average man yesterday', 'redness is not red'. These examples should be enough to indicate the sort of statements or pseudo-statements that I shall be concerned with, and I shall refer to them as *x* utterances. The question is whether they mean anything or not. The proper method, as it seems to me, to be adopted in such an enquiry is something like the following. One should take some *x* utterances and compare them first with statements that everyone would regard as meaningful and then with some that are clearly meaningless in order to see with which of these they have most affinity. One might for example take 'virtue was not much in evidence among the rulers of Nazi Germany' and 'virtue is not but', and then compare the problem case 'virtue is not square' with each of these in turn to see which it most resembles. In a recent article,¹ however, Professor A. N. Prior does not do this at all when he is defending the view that *x* utterances are meaningful. What he does is to assert that *x* utterances are meaningful, that just as from long acquaintance with grass we know that it is not red or square, so from long acquaintance with virtue we know that virtue is not red or square either. The only difference between the two cases is that we know that virtue has not got any shape or colour at all, whereas in the case of grass we know that it has a different colour and shape. He then raises what he says is the only serious difficulty about this view, and proceeds to dispose of this difficulty. This is the familiar difficulty that led Russell to formulate the theory of types. Now I do not want to object to Prior's way of disposing of this difficulty. The point is rather that not all those that oppose him on this

¹ "Entities", this *Journal*, Dec., 1954.

matter have done so merely because they can see no way of getting over the obstacle that he seeks to remove. Prior clearly feels that any reasonable person would be prepared to accept his view but for the fact that it runs into trouble. The only thing needed therefore to establish his case is to show how this trouble can be avoided. There is no need to support his own position in any very positive manner.

Now here it is clear that Prior is mistaken. In many cases philosophers have not been driven into holding, against all their immediate inclinations, that *x* utterances are without meaning for the reason he gives. Rather it is just not at all obvious on the face of it whether 'virtue is square' is meaningful or not. It certainly strikes one as a very odd sort of statement and one that one would never ordinarily want to make. Its peculiarity might consist in the fact that it is so very obviously false, as Prior would have us believe, or it might be that it is odd in the way that 'virtue is but' is odd. What its oddity is due to is a question that we can ask without, at the start, having any prejudice either way. We must ask whether *x* utterances are more like obviously meaningful or more like obviously meaningless ones and see what we find.

What then are the likenesses between *x* utterances and clearly meaningful ones? What can be said on Prior's side? As far as I know there is only one thing that has been held to be common to both of them and not shared by clearly meaningless forms of words. This common property is conformity to grammar. The former two sorts of utterance obey the rules of grammar, the latter do not. At first sight this seems a fairly convincing argument. It seems plausible to suggest that what distinguishes a meaningful from a meaningless form of words is conformity to grammar, and that *x* utterances, being grammatical, are therefore meaningful. The more one examines it, however, the less convincing this suggestion appears, and the less possible it becomes to attach any fundamental importance to grammatical conformity.

In the first place the notion of a statement's being grammatical or ungrammatical is not so clear as one tends to imagine. What are these rules of grammar that are broken by 'virtue is but' and kept by 'virtue is square'? One might expect that one would be able to look up books written by grammarians and find these rules set out somewhere. In fact, however, if one does look up such books, one finds no such thing at all. There is nothing in them at all that would enable one to decide whether 'virtue is blue' is grammatical or not. Grammarians do not seem to have

given much attention to forms of words of this type. It is not the sort of thing they are interested in. One does indeed find rules of grammar and examples of ungrammatical statements that break these rules. Unfortunately, however, the statements that break these rules are ones that are quite clearly meaningful. It is of course quite obvious that people speak ungrammatically and yet are quite well understood. The statement 'only five people lives in New Zealand' is ungrammatical, and yet we all understand it and know that it is false. Grammatical rules are mainly concerned with the contrast between correct ways of saying things and incorrect ways of saying the same things, not with distinguishing ways of saying things from ways of failing to say anything.

There are then two preliminary difficulties in the notion of conformity to grammar as a criterion of meaning. If one is thinking of the ordinary sort of rule like 'a plural subject takes a plural verb', then that sort of rule is no good as a criterion. If on the other hand you have in mind a different type of rule, one whose breach makes a sentence grammatically meaningless as opposed to grammatically incorrect, then the trouble is that rules of this sort have not been formulated.

However, I do not want to lay any stress on these objections, since there are other more fundamental ones. Moreover, it is easy to see what the rules in question would be like even if they have not been formulated, and how the idea of there being such rules arises. Suppose for example that you want to point out what is wrong with the statement 'virtue is but', then you would naturally say that the word 'but' is a conjunction and that a conjunction cannot be predicated of another word. Similarly what is wrong with 'but is happy' is that a conjunction cannot be the subject of predication, and 'unhappy is distressing' can be ruled out because 'unhappy' is an adjective and an adjective cannot be the subject of predication either. On the other hand one might say that 'virtue is blue' is admissible because 'blue' is an adjective and adjectives can be predicated of other words. Now whether or not grammarians have actually formulated a set of rules whose breach makes a statement meaningless, we can at least plausibly make the above remarks. And this suggests the possibility of formulating a systematic and complete set of the required rules. There is indeed no good reason to suppose that this would not be possible. Only, if such a set of rules were produced, their existence would be simply irrelevant to the question of whether *x* utterances are with or without meaning.

Before seeing why this is so, it is necessary to have a clearer idea of what these rules would be like. It is fairly clear that they would be based upon the classification of words into parts of speech, substantives, verbs, conjunctions and the like. For it is supposed to be because 'but' is a conjunction that 'virtue is but' is meaningless, and because 'blue' is an adjective that 'virtue is blue' is meaningful. To frame these rules, then, one would first require definitions of the various parts of speech, and then the rules would be formulated in terms of them.

What form, then, would these definitions take? How in fact do grammarians classify words as belonging to the various parts of speech? There is, unfortunately, no straightforward answer to this question, because the basis of classification is a subject of controversy among grammarians. Different grammarians recognise different numbers of parts of speech, and disagree about what should be taken into account in making the classification. Some hold that only form and form changes should be taken into account, and that what a word means should not. Jespersen thinks that three things which he calls form, function and meaning should all be considered. Others are inclined to despair of parts of speech altogether. Fortunately it is not necessary to go into details about this for our present purpose. It is sufficient to divide definitions of parts of speech into two sorts:

1. Those which define parts of speech in terms of the way words can and cannot be combined with each other in order to make sense. For example 'an adjective is a word that can be predicated of another word but cannot itself be the subject of predication' (Entwhistle), or 'a conjunction is a word that can only be used to conjoin words or groups of words' (Hall and Sonnenschein).

2. Those which make no mention of how words can be combined. For example 'a verb is a word that has tenses'. With this type of definition I shall include the possibility of leaving parts of speech undefined, and teaching the meanings of such words as 'verb' and 'noun' by pointing to examples. This, with the aid of rough definitions, is how we learn what these words mean at school.

Let us then suppose that our parts of speech have been successfully defined in the second of these two ways, and that these definitions correspond to our ordinary notions of what words belong to what parts of speech. Or let us suppose that, without the aid of definitions, we are able to say in any given case what part of speech a word belongs to. We then set about the second part of our task, which is to formulate rules that will

enable us to distinguish meaningful from meaningless combinations of words. Is it likely that we shall be successful? Shall we find that the parts of speech we have distinguished will enable us to formulate the rules we require? There seems no reason to suppose that we should. Why should we suppose that the classes of words we have distinguished on a variety of grounds and not solely on the basis of how words can be combined should be just those we need to formulate our rules? In fact it is clear that if we stick to the usually accepted parts of speech we shall not be able to rule out some even of those forms of words that are clearly meaningless. It is true that we can rule out 'virtue is but', as we have seen. But what about 'Jack but Jill went up the hill'? There is clearly something wrong with the word 'but' here. Yet we cannot say what is wrong with it if we confine ourselves to the parts of speech that are ordinarily recognised. Normally we do not regard the words 'but' and 'and' as belonging to different parts of speech. They are both conjunctions. So one cannot say that 'Jack but Jill went up the hill' is meaningless because 'but' is a conjunction, because 'and' is a conjunction too, and 'Jack and Jill went up the hill' is quite satisfactory. Let us take another example. The grammarian Sweet gives the following definition of a pronoun: 'a pronoun is a substitute for a noun and is used partly for the sake of brevity, partly to avoid repetition of a noun, and partly to avoid the necessity of definite statement'. According to this definition the word 'that' is a pronoun in the following sentence: 'his house is bigger than that of his neighbour'. 'That' is a pronoun here because it is a substitute for the noun 'house'. However we do not obtain a meaningful sentence if *any* pronoun is substituted for 'that'. The word 'that' cannot be replaced by 'her' or 'whom'. Yet this has nothing to do with the fact that they are the parts of speech that they are, namely pronouns, since 'that' is equally a pronoun.

Now it might be argued that this is not a very serious difficulty. All that one needs to do in order to get over it is to distinguish a few extra parts of speech. One might divide the class of conjunctions into two sub-classes and call each of these a separate part of speech. Some conjunctions conjoin nouns or noun phrases and some conjoin sentences. Let us call these N-conjunctions and S-conjunctions. The word 'and' can be used as both sorts of conjunction, the word 'but' is only an S-conjunction, and that is why the locution 'Jack but Jill went up the hill' is not a legitimate one. Similarly grammarians do distinguish different sorts of pronouns. There are relative pronouns like

'who' and personal pronouns like 'she', and the phrases 'than she of' and 'than who of' are meaningless because 'she' and 'who' are personal and relative pronouns respectively. One might give the pronoun 'that' a special name and say that the phrase 'than that of' is meaningful because 'that' is such and such a sort of pronoun. Now I do not see any objection at all to proceeding in this manner. If the rules are to be produced, then one has just got to proceed in this way. The point is that the parts of speech normally distinguished are not adequate tools for this particular job, so that we have to furnish ourselves with extra categories as may be required. But this is a point of the utmost importance, for if one is allowed to distinguish as many parts of speech as one wishes and is not bound by the traditional categories, then one will be able to rule out as meaningless any statement that one wishes to rule out. It will be possible to produce rules that exclude 'virtue is blue' in precisely the same way as 'virtue is but' can be excluded. It is true that 'virtue' is a noun and that 'blue' is an adjective, and it is true that adjectives can be predicated of nouns. But can any adjective be predicated of any noun? There are different sorts of nouns and different sorts of adjectives. Grammarians themselves recognise the distinction between concrete and abstract nouns. Perhaps the rules that apply to concrete nouns do not apply also to abstract nouns. Similarly why may one not have different categories of adjectives? One might recognise colour adjectives as a sub-class of adjectives. It would then be possible to have the rule 'colour adjectives cannot be predicated of abstract nouns but only of concrete nouns'. This would dispose of 'virtue is blue'. One might have a wider and more general rule. Suppose that one divided adjectives into two classes, concrete and abstract, in the way that nouns are divided. The word 'hard' in the phrase 'hard work' would function as an abstract adjective, whereas in the phrase 'hard table' it would be concrete. One could then make it a rule that concrete adjectives cannot be predicated of abstract nouns. In accordance with this rule one could say that 'virtue is blue' and 'virtue is square' are grammatically meaningless because 'blue' and 'square' are concrete adjectives and 'virtue' is an abstract noun. On the other hand 'virtue is troublesome' does not break the rule because in it 'troublesome' functions as an abstract adjective.

The position that we have now arrived at is this. One may classify words as belonging to certain parts of speech without bearing exclusively in mind whether the parts of speech distinguished will enable one to produce rules for determining the

meaninglessness of statements. If one does this there is no reason to suppose that the categories arrived at will be sufficient for the formulation of the rules. Alternatively one can distinguish one's parts of speech with a view to laying down the rules, in which case it will be possible to lay down a rule that will dispose of 'virtue is blue' in just the same way as 'virtue is but' is disposed of.

This brings us to the most serious objection to saying that it is obedience to a certain set of grammatical rules that makes a statement meaningful. The trouble is that before one can begin to formulate such a set of rules one must already know which statements are meaningful and which are not. We have seen that one will only know how many parts of speech to distinguish if one has already decided which forms of words to rule out. Furthermore, even if one did arrive at one's parts of speech independently of considerations of meaning, one would still have to know what statements are meaningful in order to know what rules to lay down in terms of these parts of speech. It is not grammatical rules that are fundamental but the meaningfulness or otherwise of forms of words. It is not necessary first to know what the rules are before one can pronounce upon the meaningfulness of a given form of words. Rather one has to know which forms of words are meaningless in order to decide if a given rule is a correct one or not. Supposé that a certain rule is proposed. How can one test its validity and decide upon its acceptance or rejection? Only by seeing if it rules out as meaningless those forms of words one regards as meaningless. Suppose for example that I formulate a set of rules and then find that the sentence 'virtue is quickly' is in accordance with them. I shall then say, not that 'virtue is quickly' must be meaningful because it obeys the rules, but that the rules must be defective. I shall try to amend them accordingly. Similarly if I regard 'virtue is blue' as meaningless, I shall not be satisfied with a set of rules that admits it. If, therefore, one wants to support the claim that 'virtue is blue' is meaningful, it is no good appealing to a set of grammatical rules. An opponent can always produce a different set of rules and appeal to those. It may be that this opponent finds it necessary to distinguish a rather greater number of parts of speech than are ordinarily recognized, but this is not surprising, since it would be necessary to do this to exclude such obviously senseless expressions as 'than who of'. In any case there is no reason why one should be restricted to a set of categories that were not designed for the task of distinguishing the meaningful from the meaningless.

So far the assumption has been that parts of speech have been defined in the second of the two ways that were distinguished above. Suppose, however, that definitions were of the first type. In that case the objections that have been raised to regarding rules of grammar as fundamental would still apply though in rather a different way. With this type of definition some of the rules of grammar would become necessary truths. If for example a conjunction were defined as a word that can only be used to conjoin words or groups of words, then the rule that a conjunction cannot be the subject of predication could not be denied by anyone whatever his views were about meaning. Disagreement about meaning would, however, show itself in another way. Suppose that, owing to some prejudice, someone thought that 'virtue is but' were meaningful. He could not deny the necessary truth that a conjunction cannot be predicated of another word, but he could deny that the word 'but' is always used as a conjunction. He could argue thus: a conjunction is a word that cannot be predicated of another word. This follows from the fact that it is defined as a word that can only be used to conjoin words or groups of words. The statement 'virtue is but' is meaningful and in it 'but' is predicated of 'virtue'. 'But' therefore can be predicated of another word and is therefore not always used as a conjunction. This shows that if parts of speech are defined in such a way that rules of grammar become necessary truths, they have to be supplemented with lists showing which words belong to which parts of speech. Otherwise they will not serve to mark off certain forms of words as meaningless. One would need to be told that 'but' and 'since' are conjunctions, 'blue', 'square', and 'virtuous' adjectives, and so on. Many words would come under more than one heading. 'Before', for example, would come under the heading 'preposition' because the phrase 'before breakfast' is meaningful, under the heading of 'adverb' because 'I have done it before' is meaningful, and under the heading 'conjunction' because 'he came before I did' is meaningful. The difference between two people who had divergent views about meaning would appear as a disagreement about what words should go under what headings. For example anyone who thinks that 'virtue is but' is meaningful will put the word 'but' under the heading 'adjective' as well as under the heading 'conjunction'. A man who thinks that 'virtue is blue' is meaningful would put 'blue' under the two headings 'abstract adjective' and 'concrete adjective', whereas anyone who disagrees will put it only under the latter heading. Thus, whichever way parts of speech are defined, grammatical rules cannot be regarded as fundamental and cannot be appealed to in a dispute about meaningfulness.

The very question at issue is what rules are the correct ones. The rules themselves can at best be a systematic formulation of one's pre-existing ideas about meaning.

This would seem to be sufficient to dispose of the view that conformity to grammar is a point of likeness between *x* utterances and clearly meaningful statements that justifies us in regarding the former as meaningful. Nothing has so far been said to support positively the contrary opinion either. The question is still quite open. What we have said does, however, do something for one who holds that *x* utterances are without meaning. It enables him to explain what he would regard as a natural prejudice in favour of distinguishing sharply between a statement like 'virtue is blue' and one like 'virtue is but'. The reason for the prejudice would be that we have learnt at school a certain set of grammatical categories made by grammarians for their own purposes, to bring some sort of order into the things they have to say about various languages. We find that we can easily frame an objection to 'virtue is but' by using these categories. We find that we cannot frame such an objection to 'virtue is blue' because we do not happen to have been brought up to make the distinction between abstract and concrete adjectives. This makes us feel that 'virtue is blue' is meaningful and quite different from 'virtue is but'.

Let us then see if there is anything positive to be said on the opposite side, in support of the view that *x* utterances are without meaning. There is one argument that is based on the familiar fact that, in the case of some words and phrases at least, their meaning cannot be learnt independently of the sentences in which they are used. One has to learn their meaning by learning the meaning of the sentences in which they are used, and, most importantly, one cannot teach their meaning by giving the meaning of any sentence in which they occur and which would ordinarily be regarded as grammatical. A clear example of an expression to which this applies is 'the average man'. How do we come to learn what this means? We are given a statement containing the expression and then told what the statement means. For example we might be told that 'the average man weighs ten stone' means 'the sum of the weight of all men divided by the number of men equals ten stone'. Having been given this example and one or two more, we become able to understand a large number of statements about the average man even though we have not been given translations of them. We can make our own translations in accordance with the correct pattern. We can translate and so we can understand what 'the average man

has 2.3 children' means. To learn what the expression 'the average man' means is to learn a pattern of translation. However, there are many possible combinations of words that would ordinarily be considered to be grammatical, and in which the expression 'the average man' occurs, which do not yield to the normal method of translation. What for example does the sentence 'I met the average man yesterday' mean? If I try to translate this I am quite at a loss. There are no numbers involved and so a translation is impossible. Why should it be supposed that just because I have learnt what 'the average man weighs ten stone' means I should also know automatically what 'I met the average man' means? The idea is quite a preposterous one. Now this argument applies very convincingly to a technical expression like 'the average man', and so shows that some x utterances are meaningless. It does not apply equally clearly to other x utterances containing less technical expressions. Nevertheless a good case can be made out for saying that this type of argument has a wider application. Can it be applied to 'virtue is blue'? One might argue in the following manner. We come to learn what the word 'virtue' means, and we do this by learning what statements like 'the Nazi leaders were not virtuous' mean. We then learn to put this another way and say 'virtue was not practised by the Nazi leaders', and so come to understand the meaning of the word 'virtue'. But in all this there is nothing that we have learnt that will enable us to understand what 'virtue is blue' means. Now this type of argument certainly needs qualification and elaboration. For example it is undoubtedly true that one could learn the meaning of the word 'virtue' without having previously learnt what 'virtuous' means. It is true that people who have learnt what 'the Nazi leaders were not virtuous' means can at once understand what 'virtue was not practised by the Nazi leaders' means without being explicitly told that it means the same as the former sentence. But this is because they have previously learnt to use other abstract nouns in a similar fashion. They have learnt that, say, 'temperance is practised by Methodists' means 'Methodists are temperate'. Provided then that they know that 'virtue' is the abstract noun formed from the adjective 'virtuous', they can understand what 'virtue is practised by Hindus' means without further explanation. But here again there is no similar parallel that will enable anyone to understand 'virtue is blue'.

At this point it is necessary to introduce a complication. So far I have confined myself to affirmative x utterances and have avoided the negative versions of these. I have considered 'virtue

is blue' but not 'virtue is not blue'. The reason for this was to avoid discussing a third alternative to the two views about *x* utterances that have so far been considered. So far it has been assumed that there are just two views: either *x* utterances are meaningful or they are not. There is, however, a third view, which was held, I believe, by Wittgenstein. This is that *x* utterances are meaningful in the negative and meaningless only in the affirmative. 'Virtue is blue' is meaningless, 'virtue is not blue' is meaningful. Now this appears on the face of it a difficult position to maintain. For these statements appear to be contradictory, and if this is so how can one be meaningful and the other not? However, this position does have a certain advantage over the view that both of these statements are without meaning. For if one holds this latter opinion it becomes difficult to explain why it is so natural to believe not only that both negative and affirmative *x* utterances are meaningful, but also that the former are certainly true. Virtue, one feels, is certainly not blue. It is possible, as we have seen, to explain why it is a natural illusion to believe that *x* utterances are meaningful; it is due to our familiarity with certain grammatical categories. But whence comes the further illusion, if it is one, that 'virtue is blue' is false and 'virtue is not blue' true? If both are alike in being meaningless, it is difficult to see why one of them should seem to be true. If, however, only 'virtue is blue' is meaningless, this difficulty does not arise.

How, then, is it possible to maintain the view that 'virtue is not blue' is true and meaningful, whereas 'virtue is blue' is without meaning? The answer to this is to say that 'virtue is not blue' is itself a disguised rule of grammar. It is necessary to explain therefore what I take to be meant by saying that this statement is a rule of grammar. Whether this is what Wittgenstein meant by calling such statements grammatical I do not know. Certainly he appears to have called some statements rules of grammar that are not rules of grammar in quite the way I want to use this expression. According to G. E. Moore,¹ he held that statements to the effect that certain colours are primary colours are rules of grammar. This, it seems to me, requires an extension of the notion of grammar that is not necessary in order to hold that 'virtue is not blue' is a statement of grammar. For this statement can plausibly be held to be one of grammar without any extension of this notion at all. 'Virtue

¹ "Wittgenstein's Lectures in 1930-33". *Mind*, 1954, 1955.

is not blue' is a statement of grammar in just the same sense as 'the word "but" cannot be predicated of the word "virtue"' is a statement of grammar.

Now it must be admitted that 'virtue is not blue' does not look like a statement of grammar. Such a statement should be about words, but this one seems to be about virtue and blue, not about the words 'virtue' and 'blue'. This, however, does not carry much weight because it is a commonplace that a statement may be verbal even if it does not look verbal. If for example I meet the expression 'elasticity of demand' in a text-book of economics, and want to know what it means, I may put my request in the form 'what is elasticity of demand?', which does not look verbal, as well as in the form 'what does "elasticity of demand" mean?', which does look verbal. It would seem, therefore, that if 'virtue is not blue' is a statement of grammar it ought to be possible to put it in a way that makes it look like such a statement. Before doing this, however, it may be more instructive to do the reverse, to take a statement that clearly is a statement of grammar and make it look unlike a statement of grammar.

Let us take the statement 'it is incorrect to say "he is the man from who I received it"'; one should say "he is the man from whom I received it"'. This is clearly a statement of grammar. Can it be put in a non-verbal-seeming way? What is needed here is to find for this statement of grammar a statement that holds the same relation to it that 'what is elasticity of demand?' holds to 'what does "elasticity of demand" mean?'. Now in a sense there is no such analogue. We have no way of making this statement about the word 'whom' without putting the word in inverted commas. This, however, is just a linguistic accident. We can easily invent such a way. The required statement would be 'you do not receive presents from who but only from whom'. Now one might conceivably express many rules of grammar in this form. If someone says 'virtue is but', instead of replying "'but" cannot be predicated of "virtue"', one might say 'virtue cannot be but'. If we were in the habit of expressing ourselves in this way and were quite used to saying things like 'there cannot be five man in a room but only five men', then it would not seem a very odd suggestion to say that 'virtue cannot be blue' is also a statement of grammar. In fact rules of grammar *are* sometimes put in this way in colloquial speech. I have more than once heard the following statement made: 'things are not different to each other, but different from each other'.

Now if we did habitually speak in this fashion it would be possible to make two lists of the things that virtue cannot be, of those things that are grammatically impossible and of those that are empirically impossible. In the first list would be such statements as:

Virtue cannot be quickly.

Virtue cannot be but.

Virtue cannot be since.

Virtue cannot be on Saturdays.

In the second list would be found:

Virtue cannot be prevalent where men are starving.

Virtue is troublesome.

Virtue is never to be found in a man who enjoys absolute power, which always corrupts.

The question is to which of these two lists the statement 'virtue is not blue' belongs. The following consideration seems to indicate that it belongs to the first list. There is no doubt that virtue is not blue. No one, as far as I know, has ever suggested that perhaps virtue is blue and that we may be mistaken in thinking that it is not. This is just what we should expect if it were a statement of grammar and just what we should not expect if it were a factual statement like 'grass is not red'. For there is no doubt about the other grammatical statements either. We know very well that virtue is not but or quickly or however. But if 'virtue is not blue' were an empirical statement about the non-linguistic world, we should have no reason to suppose that it is true. For how do we tell if something is blue or not? One might put it in white light and have a look at it. But on the view that 'virtue is not blue' is a true empirical statement, it would be impossible to perform this experiment. For on this view 'you cannot shine a light on to or look at virtue' would presumably be a true statement also. But if one cannot do this then one cannot use this way of finding out whether virtue is blue or not. It is also clear that any other way of finding out the colours of things could not be used either in the case of virtue for similar reasons. So if 'virtue is not blue' were an empirical statement like 'grass is not red', we could not possibly know what we undoubtedly do know, namely that virtue definitely is not blue.

Another thing one would expect if an *x* utterance were a statement of grammar would be that the corresponding command would be meaningless, whereas on the opposite view of *x* utterances one would expect it to be impossibly difficult to obey. Let

us therefore take an *x* utterance to which there does correspond a command, for example 'you cannot hit virtue with a hammer'. The corresponding command is 'hit virtue with a hammer'. The impossibility of obeying this command may either be due to the fact that it is too difficult or to the fact that it is meaningless. In order to see which of these two alternatives is the case, let us take an example of a clearly meaningless command and of one that is impossibly difficult to obey in order to have some means of comparison. As an example of a meaningless command I shall take an example of Wittgenstein's. This command is clearly meaningless because it contains a word that has no meaning. A king wishes to get possession of the goods of one of his subjects. He therefore sets him various tasks to perform, saying that the penalty for failure will be death. The subject carries out a number of these tasks successfully. The king then sends for him and tells him 'fetch me a clemunk'. ('Clemunk' is a nonsense word.) The subject is dejected and says 'I cannot possibly do that'. The rest of the story does not concern us. The question is why the subject could not obey the command. The reason was quite clearly that the command was a meaningless one. The case would have been different if the king had told him to stand for two hours on his little finger. In that case the command would have been intelligible but too difficult. To which of these commands is 'hit virtue with a hammer' most similar? It would seem to be most like the former. For if faced with either of these orders one would be entirely at a loss. One would not even know how to try to obey either order. On the other hand one could at least try to obey the order to stand on one's little finger even if one were sure to fail; one would at least know what one was supposed to try to do. There is of course a difference between the two meaningless commands. One is meaningless because one of the key words in it is meaningless. The other is meaningless because 'hit' is a concrete verb and 'virtue' an abstract noun. A better parallel to 'hit virtue with a hammer' would be 'fetch me a quickly' or 'hit the table with a but'. The king could have accomplished his purpose without resorting to nonsense words by giving either of these commands instead.

It may here be objected that, although these arguments do show that 'virtue is not blue' is not a contingent statement about virtue, yet they fail to show that it is a statement of grammar. There is another possibility, namely that it is a necessary truth. For if this were so it would not be surprising that we can know that virtue is not blue even though we cannot apply to virtue

the usual tests for colour. We know that a surface cannot be red and green all over at the same time, but we do not need to examine any surfaces in order to find this out.

One way of attempting to answer this objection would be to argue that all necessary truths are statements of grammar or at least statements of some sort about words. For, if this could be maintained, then 'virtue is not blue' could be both a necessary truth and a statement of grammar at the same time. But such a view cannot be maintained and is open to some well-known objections. One of these is that a statement about words may cease to be true if a change is made in the conventions for the use of the words that it is about. Therefore, if a necessary truth is about words, it too would cease to be true if such a change were made. But this is not so. The only result of such a change would be to make a form of words that formerly expressed a necessary truth no longer express this necessary truth but something else, perhaps a contingent falsehood. It could not make a necessary truth false. Another objection is that necessary truths expressed in English can be translated into a foreign language. We then have two sets of words both expressing the same necessary truth. But the English set of words, if it expresses a proposition about words, can plausibly be held to express a proposition only about English words, while the French set of words can say something only about French words. Therefore if they both express a proposition about words they cannot express the same proposition. But they do express the same proposition since they are translations of each other. Therefore they cannot be about words.

The importance of these arguments for our present purpose is that they can be applied to 'virtue is not blue' in order to show that it cannot be about the words 'virtue' and 'blue'. For it may be said that the only result of a change in the conventions for the use of these words would be to make the words 'virtue is not blue' express a different proposition from the one they do now express. This new proposition might indeed be false, but the original proposition would remain as true as it ever was. Similarly it may be argued that the correct French for 'virtue is not blue' is 'La vertu n'est pas bleu' and that this shows that this statement cannot be about words.

However, we have already given reasons for regarding 'virtue is blue' as meaningless. But this is incompatible with the view that 'virtue is not blue' is not about words. For if it were a necessary truth and not about words, then 'virtue is blue'

would not be meaningless but necessarily false. Therefore either our original reasons for thinking that 'virtue is blue' is meaningless are not good ones, or the arguments we have just given for the opposing view are not as cogent as they seem.

Let us then see if these arguments are conclusive. The second argument is certainly not conclusive. It simply assumes that the correct translation of 'virtue is not blue' is "*la vertu n'est pas bleu*". But this can be denied. For if one assumes this one might equally well assume that the correct translation of the question 'What is an encomium?' is always '*qu'est ce que c'est qu'un éloge?*' But it is clear that this question may be asked by someone who does not know what the word 'encomium' means, and be a request to have the meaning of it explained. It is sometimes a way of asking 'what does the word "encomium" mean?' When the words 'what is an encomium?' are used in this way, then the French equivalent is just not '*qu'est ce que c'est qu'un éloge?*' It would be absurd to argue that, since this is the French equivalent, the English version cannot be a query about the meaning of an English word. Sometimes it just is such a query and therefore it cannot mean the same as the French sentence which is equally clearly not such a query. Again suppose that it were a linguistic fact that the words 'virtue is not but' were used to make a contingent statement of English grammar in the way described above, and suppose that it were also a fact that the words '*la vertu n'est pas mais*' were used to make a contingent statement of French grammar, then these two statements would not mean the same as one another. They would indeed look like translations of each other, and you would arrive at one as a translation of the other by translating blindly according to the procedure that is usually successful. But in fact they would be two different statements, one of English and one of French grammar, both of which happened to be true because of a certain similarity between the French and English languages. Indeed one can envisage a language which was so similar to English in the relevant respect that even the statement 'things are not different from but to each other' would appear to be translatable into a sentence that used or mentioned no English words at all. Such a language would be one into which any statement (apart from those of the type now under discussion) could be translated by substituting for each word whenever it occurred a certain word in the language. One could translate from English into this language quite mechanically if one had a list of all English words with the appropriate word in this language opposite each of them. Translation would simply consist in replacing each English word by the word opposite it in the list.

However, if one used this method of translating 'things are different from not to each other' one would indeed get a true statement in the other language, but it would not be a translation. This would be proved by the fact that if it were a correct translation then the English sentence could not express the proposition of English grammar that it undoubtedly does express. To return to 'virtue is not blue', it cannot be assumed that 'la vertu n'est pas bleu' is the correct translation into French. For whether it is or not will depend upon whether 'virtue is not blue' is a contingent verbal statement or not. If it is, then the suggested translation is the wrong one. Clearly the way a sentence should be translated depends upon what it means, and it is ridiculous to make an inference about what it means from the way it is translated.

What then of the other argument? This is to the effect that, since no alteration in the meaning of the words 'blue' and 'virtue' would render untrue what is expressed by the words 'virtue is not blue', these words cannot be used now to express a proposition about 'virtue' or 'blue'. We may first note that this argument, if valid, would apply equally well to the statement 'virtue is not but'. If, for example, 'virtue is not but' came to mean 'virtuous people never have doubts', then it might be argued that this would not make untrue what is now expressed by 'virtue is not but'. There would indeed be a new sense of 'but' in which 'virtue is but' expressed an empirical proposition about people, but in the old sense of 'but' it would still be true that virtue is not but. The fact that this argument applies to 'virtue is not but' makes it seem likely that it must be possible to answer it.

How, then, might one try to answer it? One possibility would be simply to deny that no change in the meaning of 'blue' and 'but' would affect the truth of what is expressed by 'virtue is not blue' and 'virtue is not but'. One might argue that if, for example, 'virtue is blue' came to mean 'the practice of virtue makes men sad' then it would no longer be true that virtue cannot be blue, and that if 'virtue is but' came to have the meaning suggested above then it would no longer be true that virtue cannot be but. To take this position would be to treat these forms of words as expressing propositions that are contingent and about words. Now there is no doubt that they could be used in this way, though it might be denied that they are so used in fact. Whether they are so used in fact is a question we shall return to shortly.

There is, however, another way of meeting the objection we are considering. One can admit that no change in the conventions of the English language would make it false that virtue is not but or blue, and still maintain that this statement is about words, by maintaining that it is a necessary truth about words. For there is no reason why, in the sense of 'about' in which the statement that all giants are tall is about giants, there should not be statements about words. What is needed is a statement about words that is independent of any contingent facts about any language. Let us take the form of words "But" cannot be predicated of "virtue". Can this be interpreted as expressing such a proposition? A step in this direction can be taken by regarding the word "but" as standing for the word 'but' not in any sense of that word, but only as the name of the word in so far as it is a conjunction. If "but" is taken in this sense, then even if 'virtue is but' acquires a meaning it will still be true that "but" cannot be predicated of "virtue", because, in so far as 'virtue is but' does have a meaning, 'but' is being used as an adjective. This sense of "but" cannot be predicated of "virtue" would be more clearly expressed by the words 'the conjunction "but" cannot be predicated of the abstract noun "virtue"'. This sentence, however, if taken in its most natural sense, is still not entirely independent of contingent linguistic facts, since it presupposes that there is a conjunction 'but' in the English language. It is rather like 'the man next door is next door' which is not quite independent of the contingent facts about who is next door because it presupposes that there is a man next door. It is possible, however, to achieve complete independence of anything contingent by taking 'the conjunction "but"' in a rather less natural way. We might interpret it as meaning the same as 'the adversative conjunction', that is as a description that applies equally to 'but' in English, 'mais' in French, 'sed' in Latin, and so on. This would be like using 'the article "the"' instead of 'the definite article'. If this were done the statement 'the conjunction "but" cannot be predicated of the abstract noun "virtue"' would be quite independent of any contingent linguistic facts. Now we might interpret 'virtue is not but' as being a way of saying the same thing, in which case it too will be independent of contingent linguistic facts. And if a parallel procedure were adopted in French then the French for 'virtue is not but' would be 'la vertu n'est pas mais'. It is also clear that *mutatis mutandis* a similar case could be made out for regarding 'virtue is not blue' as a necessary grammatical truth.

To recapitulate, we have mentioned four accounts of the statement 'virtue is not blue'. It can be taken as:

1. A contingent statement about virtue.
2. A necessary statement about virtue.
3. A contingent statement about the words 'virtue' and 'blue'.
4. A necessary grammatical truth.

We have given reasons for rejecting the first two alternatives. We have not yet considered which of the last two is to be preferred, which of these is the correct account of how this sentence is in fact used. Neither answer would seem to be particularly plausible, and the reason for this is that both ascribe too precise a meaning to the sentence. The position is rather something like this. In the normal course of events we never use this sentence at all. When, however, it is made it is natural to assent to it, and this is because we feel that there is something wrong with the statement 'virtue is blue', and it is natural to express our rejection of it (without reflection or analysis) by saying 'virtue is *not* blue'. All that can be said about the actual use of this form of words is that it is such a rejection. What *can* be done is to ask what are the grounds for rejecting 'virtue is blue', and I have argued that the ground is that it makes no sense. It is perhaps a little misleading to ask this question in the form 'what does "virtue is not blue" mean?', which is what has been done in this article. However, once we have decided what the grounds for rejecting 'virtue is blue' are, it is possible to go on to suggest more precise meanings that can be given to the sentence 'virtue is not blue' while still preserving its role of rejecting 'virtue is blue' as meaningless. The two alternative accounts of the statement 'virtue is not blue' are best regarded as suggestions of this sort. This is not to say that I am making any recommendation about the way this sentence should be used. On any interpretation it is a confusing way of speaking, and the best thing to do would be to drop it altogether.

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FREEDOM AND REAL WILL THEORIES

By H. J. N. HORSBURGH

The notion of a Real Will, like most of the notions introduced by the idealist school of political philosophy, is widely derided today. But philosophical snakes are more often scotched than killed; and therefore it is important to study their coils in an effort to trace the sources of that fascination which may otherwise prove fatal. Some Real Will theories are undoubtedly plausible, and in particular I think it can be shown that a common-sense approach to the question of freedom can easily lead one in the direction of a Real Will theory. Present-day writers on political idealism seem to underrate the appeal of Real Will theories (unless they accept them) and therefore disdain to uncover their linguistic and psychological roots.¹ For this reason the subject appears to merit further attention.

In this article I have therefore set myself the following tasks: (i) to expose the standing temptations to adopt some Real Will theory, and (ii) to show what the common-sense approach to freedom must embrace if it is to escape the clutches of Real Will theory.² I try to exhibit these temptations by means of a sketch of the stages through which the notion of freedom may be said to pass in the course of its development. (It is not claimed that these stages correspond to anything in the historical pattern of development.) This sketch shows at what stage, and in what way, the temptations manifest themselves. I then go on to plot J. S. Mill's position in relation to this sketch and to consider the allegedly anomalous passages in his essay 'On Liberty' that Bosanquet fastened upon. I introduce Mill as a representative of the common-sense approach to freedom and I try to determine whether he is driven—as Bosanquet alleges—at least to the brink of a Real Will theory. This is clearly a further study of the temptations to which I allude. Its other

¹ John Plamenatz's *Consent. Freedom and Political Obligation* seems to me far the most able discussion of this whole subject. Yet it does not help one to understand why men have held Real Will theories.

² Of course, 'common sense' is a very vague term. But I have been unable to avoid its use. However, I do not think that any important theoretical issue is obscured by its vagueness. Generally, the common-sense approach to freedom is mildly individualistic, liberal and unsystematic. This last and most obvious feature explains why I have relied on context to explain what I mean by it instead of undertaking a general analysis.

function is to clarify the direction that must be taken by adherents of the common-sense approach to freedom once they have decided to resist these temptations. I also try to make it clear why this direction is to be preferred, from the standpoint of freedom, to that which would have been forced on them if they had succumbed to the temptations.

I

The simplest conception of freedom is that of unreflective individualism. On this view society and the individual are opposed in the sense that any interference with the individual is seen as an encroachment on his liberty. Freedom is simply the total absence of external constraint. Such a conception is too primitive to give lodging to any of the sophisticated ideas which lead to political idealism. Thus it makes no sense on such a view to speak of being 'forced to be free'; and while it is true that not all the possible meanings of that arch-ambiguous phrase imply some Real Will theory, it is always possible to speak of being forced to be free in some sense when one has to do with a conception of a Real Will.

Unreflective individualism inevitably gives rise to questions which ensure its eventual modification. It is seen that one's freedom is threatened as much by other individuals as by society and that some protection must be found from these constant and serious menaces. It is also recognised that freedom must be conceded to other individuals. The consequence is an advance to what one might term self-qualifying individualism. Freedom is still seen as the absence of external constraint. But it is admitted that my freedom must be limited by other people's and that I can only be free at the price of accepting certain controls and acquiescing in equal freedom for other individuals. Thus, at the stage of self-qualifying individualism, one is prepared to cut down one's originally unrestricted claim to freedom so as to ensure that the remainder of one's original claim will cease to be mere claim and will become assured fact.

On this view, freedom depends on one's willingness to renounce part of one's original claim to freedom. Therefore, if one is forced to accept this arrangement one is being forced to be free. But this sense of being forced to be free does not generate or suggest any conception of a Real Will; for the coercion of the individual can be justified, not by reference to what he himself wants, or is alleged to want, but by reference to his good or interest. (Later in this article I shall have occasion to discuss this matter in greater detail.)

At this stage individualism itself begins to be questioned. The fact that it is recognised by self-qualifying individualism that one's freedom depends on the organised strength of society makes the alleged opposition of individual and society something patently hollow. But one can discern several steps between the views I have so far outlined and the first explicitly non-individualistic conception of freedom. I am only concerned with the first of these steps. This is taken when someone asks: But surely it is sometimes oneself who prevents one from doing what one wants? The man who is worried about losing his job, gets drunk, and does something which results in the loss of it, is his own enemy; and all of us are constantly standing in our own light in similar, and often far more subtle, ways. Freedom, then, is not simply the absence of external constraint; it is the absence of any constraint that prevents one from doing what one *really* wants. Thus, one's freedom requires to be safeguarded not only against others but also against oneself. And many idioms of the freedom-from variety support or imply this view, e.g., freedom from sin, from base thoughts, from distracting impulses.

This protection, which has to be extended by society to the individual to save him from himself, forces him to be free. It interferes with his conduct in such a way as to ensure that instead of indulging some base impulse he does what he really wants. Here at last we have a distinction made between what the individual appears to want and what he really wants. This is the distinction between what Bosanquet terms the actual will and the Real Will.¹

The Real Will theorist always takes his departure from the fact of psychological conflict. He notes that we are divided against ourselves, and it is these divisions which enable him to draw his distinctions. In the simpler Real Will theories all he does is to take sides in these conflicts, claiming that such and such a desire or kind of desire is more essential to the self or more representative of the individual than some other desire or kind of desire. It therefore reveals what he really wants as distinguished from what he wants at some particular moment when, as the phrase goes, 'he is not himself'. In the more complex Real Will theories the connexion with the individual's actual desires is much obscured, the conception of the Real Will having been developed to a point at which its link with the self we know, the desires we feel and seek to gratify, is vestigial. Yet some connexion must be maintained, for if the content of

¹ Of course the distinction is not made in this way by Bosanquet.

the Real Will never finds its way into the flux of a man's desires, and is not bound in any way to that flux, what possible justification can be offered for the use of the term 'will'?

All this can be made much clearer by briefly reviewing two Real Will theories, beginning with the one whose birth we have just witnessed. The Real Will, according to this view, is simply our long-term purposes or desires as opposed to our distracting short-term purposes or desires and our momentary impulses. Such a theory can confidently appeal to ordinary usage, for it would strike nobody as odd to hear someone say, "I didn't **REALLY** want to hurt him but I couldn't seem to help myself". The germ of it is also to be found in Butler. And to judge by how we speak and behave most of us agree with him when he says that some elements in our make-up have natural authority over other elements so that we can appeal to them as standing for us, as representing us, in a way that other elements do not. But the words 'real' and 'natural' used in this connexion draw attention away from the fact that the self which these elements represent is often not the individual as he is but his official self, the individual as he should be or as he likes to think of himself. In other words, what we really wanted turns out to be akin to what we should have wanted, and the Real Will is discovered to be an ideal will. But the use of the word 'real' is not altogether inappropriate and is certainly of great psychological significance. We do have the desires which we say we really want to gratify, and it cannot be denied that they have a different psychological as well as moral status from that of momentary impulses, etc., for we habitually identify ourselves with them. Therefore, they are more real to us than our short-term purposes, i.e., they are in our thoughts more frequently and for longer periods. Also, our insistence on their importance protects us from dreadful admissions concerning the strength of momentary impulses, half-recognised desires, and covert aims that issue from psychological levels with which very few of us are on thoroughly friendly terms. Thus psychological repression, the ambivalence of our desires, and much else that post-Freudian psychologists have investigated probably lie behind this usage, driving us towards the shuffle I have noted between the real (in either the sense of the actual or the subjectively important) and the ideal. In more sophisticated versions of the Real Will this shuffle becomes a smooth and unending slide, attention often being drawn away from it by high-powered metaphysics.

The Real Will can also be interpreted as that will which develops out of certain higher-order desires. It can be claimed that in addition to the particular desires of the actual will, which

vary from person to person, we all have certain higher-order desires, i.e., desires concerning our desires. Thus, we should like our particular desires to be coherent. We should also like them to have objectives that would satisfy us if we attained them. These higher-order desires show that our actual will falls short of what we, as creatures of desire, aim that it should become. They provide it with a programme or a goal, namely, a Real Will which will be what one's actual will strives to be and fails. This Will, in which all the divisions of one's actual will are overcome, is a will to a way of life which satisfies the needs and demands of a rational being. (I believe that this is what becomes of Bosanquet's view when it is forced to shed its metaphysics.) Furthermore, this Real Will coincides with the General Will, and achieves at least partial embodiment in the State. Thus it offers us a way of escape from our internal divisions, our social conflicts, and the limitations of our individual lives. Is it any wonder, then, that it is psychologically appealing when we are told that the seeds of this all-reconciling will are already planted in us and that the green fingers of the State can nourish them?

This is a far less simple view than the previous one. It is also much less plausible. Admittedly it is easy to persuade oneself that there are higher-order desires of the kind on which the conception rests. But the conclusions drawn from these assumptions are extraordinarily far-reaching. The vertebrae of the argument seem to be broken and incomplete like those of a prehistoric skeleton and one distrusts the anatomical ingenuity of the idealist reconstruction. But it is no part of my present business to make daylight pour through these gaps.

All I wish to do is to point out how much further this conception of the Real Will departs from the actual will than does the previous conception. So as to justify the use of such terms as 'will' and retain a connexion with the notion of consent (without which the political value of the conception would crumble away) some links with the actual will are insisted upon; but they have become so weak and strained as to be compatible with the individual's no longer knowing the content of his Real Will. (The previous theory was such that the Real Will could always be revealed by the individual himself; it was always "what I really want".) Such a view underlines the danger of any Real Will theory; for any gap between what the individual seems to want and what he really wants can always be widened, and the wider this gap the easier it is for other individuals or institutions to interpose themselves between the appearance and

the reality of the will, arrogating to themselves the function of interpreting the Real Will. (We have all heard domestic tyrants telling others what members of their families really want.) Of course, for the more sophisticated of those who hold such theories this is not what makes them dangerous but what confers their value: their political significance, from this point of view, lies in their capacity to open the door to authoritarian practices without appearing to do so. The Real Will soon shows itself to be the General Will,¹ and the General Will is found to be identical with the activity of the State.

These two theories, then, illustrate the division of Real Will theories into the less radical, which, in their more developed forms, are ethical, and the more radical, which are political. The latter are distinguished by the coincidence of the Real Will and a General Will. Any theory of the less radical and relatively innocuous sort can develop into a political theory through a sufficient widening of the gap between the actual will and the Real Will; and there is hardly any limit to the number of claims by which the theorist can extend this gap. Thus he can assert that the individual voluntarily blinds himself to what he really wants, that he has important desires of which he is unaware, that his desires are so chaotic that they can only be reconciled by means of some authoritative intervention, etc., etc. And many of these claims, such as those I have enumerated, can be persuasively supported.

II

"... liberty", says Mill, "consists in doing what one desires" And it is clear that he thought of this liberty as threatened only by external constraints. Equally obvious is his acceptance of social controls. His conception of freedom is therefore basically that of self-qualifying individualism.

In the following passage the fundamentals of his approach are clearly stated: "... the sole end for which mankind are warranted, individually or collectively, in interfering with the liberty of action of any of their number, is self-protection. That the only purpose for which power can be rightfully exercised

¹ Of course the term 'General Will' is at least as ambiguous as the term 'Real Will'. D. H. Monro (*Philosophy*, 1951) finds it used in three senses by Rousseau, and L. T. Hobhouse distinguishes seven in his *Metaphysical Theory of the State*. Not all the conceptions of a General Will are politically dangerous. I have those of such writers as Hegel and Bosanquet chiefly in mind. Generally, that a conception of the General Will is derived from a conception of the Real Will is a bad sign.

² *On Liberty*, p. 152, Everyman edition. This is the edition to which I refer throughout this article.

over any member of a civilised community, against his will, is to prevent harm to others. His own good, either physical or moral, is not a sufficient warrant. He cannot rightfully be compelled to do or forbear because it will be better for him to do so, because it will make him happier, because, in the opinions of others, to do so would be wise, or even right. These are good reasons for remonstrating with him, or persuading him, or entreating him, but not for compelling him, or visiting him with any evil in case he do otherwise."² In this important passage Mill seems to be making a number of vital assertions or demands, including the following: (i) that social controls over the individual are essential and may be enforced against his consent if they are necessary to the well-being of other individuals; and (ii) that social controls over the individual should not be enforced in his own interest. The latter view is implied by self-qualifying individualism since that conception of freedom can find no place for the notion of protecting the individual from himself. (Forcing the individual to be free, in the sense of forcing him to accept the social arrangements without which freedom would be impossible, is a more complex form of protection than those to which Mill is alluding in the above passage. This apparent exception to the rule is best dealt with when I come to the allegedly anomalous parts of Mill's essay.) But Mill eliminates so many alternatives with such care in this passage—the "remonstrating . . . persuading . . . entreating . . . compelling . . . visiting evil on . . ." reminding us of the word-lists with which recent philosophical writing has made us only too familiar—that I tend to think he was also guarding his conception of freedom from the subtle erosions wrought by those who extend the meaning of the word 'consent', i.e., the Real Will theorists in all their guises. Thus we are told that forcing the individual to do something because it would make him happier, better, etc., is not permissible. Mill seems to be doing his best to prevent any distinction arising between what the individual seems to want and what he really wants. It can therefore be claimed that in this passage Mill is warning off two sets of people whom he regarded as the enemies of freedom: those who brook interference with the individual in his own interest, and those who seek to deny by some theory of the Real Will that what seems to be a violation of the individual's liberty is in fact what it seems to be. There is no doubt, however, that Mill's eye is principally on the first group and that he does not explicitly attack the second.

² P. 72-73.

It is Bosanquet's contention that this conception of freedom breaks down even in Mill's text. He cites two passages in support of this view. I believe that there are three passages which suggest that Mill fell into inconsistency and I now propose to examine these in turn, beginning with that about which Bosanquet remarks, "here we have in germ the doctrine of the 'Real Will', and a conception analogous to that of Rousseau when he speaks of a man being 'forced to be free'".¹ The passage is to be found on pp. 157-158 of the essay: "In this and most civilised countries, for example, an engagement by which a person should sell himself, or allow himself to be sold, as a slave, would be null and void The reason for not interfering, unless for the sake of others, with a person's voluntary acts, is consideration for his liberty. His voluntary choice is evidence that what he so chooses is desirable, or at least endurable, to him, and his good is on the whole best provided for by allowing him to take his own means of pursuing it. But by selling himself for a slave, he abdicates his liberty; he foregoes any future use of it beyond that single act. He therefore defeats, in his own case, the very purpose that is the justification of allowing him to dispose of himself. He is no longer free; but is thenceforth in a position which has no longer the presumption in its favour that would be afforded by his voluntarily remaining in it. The principle of freedom cannot require that he should be free not to be free."

Though he does not use the words there is no doubt that Mill is speaking of being 'forced to be free', as is shown by the final sentence of the excerpt. But this is not enough to prove that Bosanquet's comment is justified, since, as I have already pointed out, the phrase 'forced to be free' does not invariably imply some conception of a Real Will. To decide whether or not this is an innocuous sense of being forced to be free requires us to interpret the whole passage, and that is less easy than it seems at first, for—as so often with Mill—the meaning of each sentence is clearer than that of the passage as a whole. However, I am pretty sure that the gist of what Mill is saying can be expressed in the following way. The individual should be free because his good is best served on the whole "by allowing him to take his own means of pursuing it". But if he uses this freedom to sell himself for a slave he is no longer free to take his own means of pursuing his good and the reason for granting him his freedom has been defeated. One must therefore distinguish between the higher-order freedom to choose between

¹ *The Philosophical Theory of the State*, p. 65.

freedom and slavery and the lower-order freedom to make the ordinary decisions which have no bearing on one's future freedom to make similar decisions. Only the latter sort of freedom can be justified with reference to the individual's welfare and therefore the higher-order freedom should be withheld, thereby forcing him to be free.

If my interpretation of Mill is correct, therefore, the sense of 'forced to be free' implied by this passage does not contain the germ of a Real Will theory. On the contrary, it is fully consistent with Mill's self-qualifying individualism and can even be said to be the only rational position open to one who accepts that conception of freedom. The following argument, from Mill's premisses, should show that this is the case. Slavery is not in the individual's interest, since this is best served by "allowing him to take his own means of pursuing it". Therefore, freedom to choose slavery cannot be justified with reference to the individual's good. But the individual's good must outweigh his freedom, in any sense of 'freedom', since at most his freedom can only be part of his good. Therefore, only those sorts of freedom are justifiable which are in the individual's interest. And so, while the freedom of allowing the individual to pursue his own good in his own way is a justifiable sort of freedom, the freedom to choose between freedom and slavery is not a justifiable sort of freedom.

The passage is also consistent with what Mill has to say about the relations between the individual's good, interest, or welfare and his freedom, for when he says that violations of the individual's freedom in his own interest are not permissible it is to the lower-order freedom that he is referring.

The next passage I wish to consider is to be found on p. 73 of Mill's essay. Here he amends what he has already said by declaring it to be inapplicable to children and 'barbarians'. "Those who are still in a state to require being taken care of by others, must be protected against their own actions as well as against external injury. For the same reason we may leave out of consideration those backward states of society in which the race itself may be considered in its nonage Despotism is a legitimate mode of government when dealing with barbarians, provided the end be their improvement, and the means justified by actually effecting that end."¹ It is not immediately clear how this passage should be interpreted. If Mill is simply making exceptions to the rule that a man's good or welfare is best served by granting him freedom it is interesting as showing that he

¹ Yet Mill says on p. 148: ". . . I am not aware that any community has a right to force another to be civilised."

recognises that he has taken too simple a view of the relations between freedom and welfare but it cannot be said to contain any seeds of a Real Will theory. (I shall have more to say concerning its implications in connexion with the next passage.) On the other hand, Mill's assertion that children and primitive men require to be protected "against their own actions as well as against external injury" could be taken to imply the emergence of the view that liberty is just as much a matter of freedom from internal as freedom from external constraints. If it is interpreted in this way Mill may be taken to have covertly introduced a simple Real Will theory, for all such views lead on to statements about what individuals or people *really* want. But the former interpretation is far less strained and is therefore to be preferred. Once again, therefore, Mill must be acquitted of Bosanquet's charge.

The passage which remains to be considered is as follows: ". . . it is a proper office of public authority to guard against accidents. If either a public officer or anyone else saw a person attempting to cross a bridge which has been ascertained to be unsafe, and there was no time to warn him of his danger, they might seize him and turn him back, without any real infringement of his liberty; for liberty consists in doing what one desires, and he does not desire to fall in the river."¹ There can be no doubt that the view Mill takes in this passage is thoroughly in accord with common sense. We think it quite proper that we should forcibly save the life of a man who steps in the way of traffic or onto an unsafe bridge; and we should all agree that officials should be ordered to act in this way. Some of us would also agree with Mill when he says that the forcible prevention of accidents does not encroach on the liberty of those who are saved from them. Faced with such coercive actions we draw attention to the peculiarities of the situation, confident that these are sufficient to justify what we have done. Thus we point out that it is only A's ignorance and the imminence of the danger which make it necessary for us to use force. We may then go on to claim that it is virtually certain that A will give his retrospective consent to what has been done.² We also enlarge on the fact that A is not even in a state of conflict over the incident. If he knew the facts he would respond to them with his whole heart and mind. But for the moment I shall resist these pleas and proceed to examine Mill's view afresh.

¹ Pp. 151-152.

² This, of course, is patently nonsense. One can give one's approval, but not one's consent, to actions which have already been carried out. The slide from approving to sanctioning and from sanctioning to consenting makes it easier for us to extend the meaning of the word 'consent'.

Because of the suddenness of the assault and the absence of an explanation A will probably resist the person who seeks to remove him from the bridge. Yet we insist with Mill that "he does not desire to fall in the river". We may have no grounds, or no adequate grounds, for this assertion. But whether we have or not, are we not saying that, although A is resisting us, thereby seeming to show that he does not wish to leave the bridge, he does REALLY wish to leave it, since he does not want to fall in the river? In other words, it looks as if we are acting as the interpreters of his Real Will. In this case there does not seem to be any reference to an interior conflict in A such as I have pointed to as the origin of Real Will theories. But although there is no actual conflict in A we perceive that his evident wish to remain on the bridge is at odds with his wish not to fall in the river. This enables us to side with his wish not to fall in the river against his desire to remain on the bridge, claiming that it represents him as the other does not. Usually we cannot take sides in this way unless there is felt conflict in the individual concerned; but in this case it is possible because we perceive an opposition that is hidden from the individual himself. We therefore assimilate this case to cases of the sort which generate the simpler of the two Real Will theories I have outlined.

But this is not the only possible interpretation of Mill's text. It might be admitted that A wants to remain on the bridge. It might then be claimed that what is being said is that he WOULD want to leave it IF he knew it was likely to fall. But one is then disposed to ask: As this account of what A would want is being advanced as a justification for hustling him off the bridge, how does it justify that action? Is it not because its effect coincides with what A REALLY wants? To the latter question the answer should be firmly negative: "No, it justifies the action because its effect coincides with what A would want if he knew the facts." In other words, we have to do not with an appeal to the individual's Real Will but to his interest. Such an interpretation is open to us because what an individual wants, and what he would like in certain circumstances, are relevant to his interest or welfare. Therefore, references to them do not inevitably imply the covert adoption of a Real Will theory.

I am therefore inclined to acquit Mill altogether of the charge which Bosanquet brings against him. But this passage, like the previous one, is nevertheless anomalous in that it shows that Mill was unable to sustain the view he meant to take with regard to the relations between the individual's welfare and his freedom. If I hustle a man off a bridge in order to prevent an

accident I am obviously infringing his liberty in Mill's sense of the word. (If Mill had not been at least dimly aware of this it is hardly likely that he would have used the phrase, "without any *real* infringement of his liberty".) But I am also acting in his best interest. What Mill should have said is that there are circumstances in which the individual's welfare, as this is understood by others, may legitimately take precedence over his freedom. Such a view would not have affected his most fundamental utterance on the relations between freedom and welfare, for this was surely the assertion that the individual's good "is on the whole best provided for by allowing him to take his own means of pursuing it". Mill's statement is qualified by the words "on the whole"; and that phrase allows for the occurrence of exceptions.

III

So far I have tried to show how Real Will theories arise, how they are embedded in ordinary usage, and the nature of the psychological temptations that lead to their adoption. I have also indicated why they are politically dangerous and how easy it is to pass from a relatively harmless to a relatively harmful Real Will theory. In addition I have examined Mill's essay and come to the conclusion that he probably did not slip unawares into the adoption of a Real Will theory anywhere in his text. I must now return to the passage I have just been examining with a view to bringing to light its general significance.

However kindly disposed one may be to Mill, it would be difficult to deny that the seeds of a Real Will theory nearly blow into his pages through the passage I am considering. That this is the case is shown by at least two things: (i) his denial that forcible removal from a bridge is an infringement of one's liberty; and (ii) his references to the wishes of the person who is being removed. The latter is of importance because the situation is one in which the stakes are as high as a man's life, and this is the hardest sort of case in which to establish a connexion—as, in Mill's interest, I tried to do in the previous section—between what the individual wants and his welfare, good, or interest. (Few would say that the saving of a man's life ceases to be in his interest or for his good when he wishes to die—except, of course, when the wish is a deep longing grounded in terrible suffering that cannot be alleviated.) Nevertheless, Mill can escape from Bosanquet's charge. But the price he must pay for his escape is a retreat from the position he has taken up with regard to the relations between freedom and welfare. However, not only is this amendment compatible with

the fundamentals of Mill's view, as I have already shown, but it is also required to bring what he says completely into line with common-sense opinion; for surely, if A is liable to be knocked down by a bus and we push him violently out of its path, we do not think that the justification of our action is dependent on A's wishes.

It seems clear, therefore, (i) that Mill has been driven beyond self-qualifying individualism, and (ii) that he is forced to choose between the adoption of a Real Will theory and the admission that interference with the individual's conduct can sometimes be justified on the ground that it is in his own interest. Clearly, both these views can prove a danger to the sort of freedom that is dear to Mill and common-sense opinion. This passage in Mill's essay therefore raises the question: Which of these alternatives puts freedom in the greater jeopardy? (Real Will theories are a species of myth-making and as such are hopelessly unsatisfactory from a purely intellectual standpoint. But the myth of a Real Will might have desirable social effects, tending to make men freer, happier, etc., than they would otherwise be. Therefore the intellectual unsatisfactoriness of such theories does not help us to answer the question I have just raised.)

There are several reasons for holding that Real Will theories are the greater menace to freedom.¹ These reasons have a common root, namely, the inability of the Real Will theorist to draw many vital distinctions. If one justifies one's actions by reference to the individual's welfare rather than his Real Will it does not obscure the fact of interference with his conduct. Interventions in the name of welfare are interventions which involve a conflict of principle; and such conflicts bring home to us the significance of our decisions, forcing us to be more scrupulous than we would otherwise be. For, on Mill's amended view, such intervention is still held, generally speaking, to be unjustifiable; and this means that whenever it occurs reasons have to be given for supposing the situation to be exceptional. On the other hand, if one adopts a Real Will view one avoids conflicts of principle and one's interference with the individual's conduct is only limited by the lesser or greater extravagance of one's theory or by the nebulous safeguards spoken of by such

¹ I shall concentrate on the most fundamental of these reasons. There are also important differences between the two views with regard to the distinction between my interest and someone else's (which some Real Will theories are forced to deny) and with regard to their respective claims to knowledge of the desires, etc., of those with whose conduct it is proposed to interfere.

writers as Bosanquet.¹ This difference is clearly of the greatest importance. The notion of welfare is also a dangerous one whose content can never be sufficiently clarified or agreed upon. Yet it does not prevent us from drawing a number of relatively simple distinctions which can be applied without undue arbitrariness to the complex flow of events and which then provide us with reasons for describing situations as exceptional. In the first place, it is sometimes clear that A cannot look after his welfare, or some aspect of his welfare, and that B or some association is competent where he is not. A's helplessness may have any number of causes, e.g., his ignorance (as in Mill's example), some relevant disability (e.g., deafness, madness, alcoholism), etc. In the second place, it is often clear—in spite of differences of opinion with regard to the nature of well-being—whether the issue is a minor or a major one, e.g., an issue of life and death or a trifling inconvenience, discomfort, etc. In the third place, it is frequently clear whether the loss that would be sustained by the individual as a result of non-intervention would be temporary or permanent and whether it would be educational. Thus the sensible father prevents his children from having serious accidents (as far as he can) but does little to prevent the minor but sometimes painful mishaps from which children learn to look after themselves.

From the standpoint of freedom, therefore, it is much less dangerous to curb the individual in the name of his own welfare than in that of his Real Will. But the former remains a very serious threat to freedom in spite of the distinctions to which I have drawn attention. However, such threats are inescapable. Indeed, it is a capital point in favour of any view that it highlights the hazards of practical affairs. No set of principles, attitudes, or institutions will automatically safeguard our freedom. There is no substitute for enlightened suspicion, courage, and the stubborn will to remain unshackled.

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¹ See Chapter 8 of *The Philosophical Theory of the State*, especially pp. 182-186, where he struggles to apply the safeguards in such a way as to make them yield enlightened answers to questions current in his day. An idealist should have known better than to descend to such undignified 'cooking'.

ON DESCRIBING THE WORLD

By JOHN HARTLAND-SWANN

I want to examine a problem which has been plaguing philosophers for a long time and which still, in this analytical age, provokes discussion. It may be stated, provisionally, as follows:

What exists independently of perceptions, and if anything does exist how shall we describe it?

Now whether or not I have stated a pseudo-problem—and it will seem such to some—this is in fact a problem which is common and indeed basic to a whole series of philosophical issues which have become familiar to us under such titles as 'The Problem of Perception', 'Our Knowledge of the External World', 'Appearance and Reality', and so forth. Thus we have here a problem—whether sensibly formulated or not—which appears to have ramifications in both metaphysics and epistemology, although strenuous attempts have been made to answer it in such a way as to eliminate all metaphysical implications. I do not think that all metaphysical implications can be eliminated; but I shall proceed on the assumption that the metaphysical aspect of the problem may be profitably neglected.

My first point is that what we are called upon to *solve* is, despite appearances to the contrary, a descriptive and not a factual problem; and that mistakes which have been made in the past have often been the result of a failure to recognize this. Of course, the problem as stated is partly a factual problem; but the persons qualified to state or investigate the facts are not necessarily or usually philosophers as such: they are (a) ordinary human beings and (b) scientists of one kind or another. The job of the philosopher is to do what neither the layman nor the scientist is specifically trained to do, namely, to describe the facts of the situation in the kind of language which will bring its logical character into proper perspective. But it is not the philosopher's job to argue about the facts, since these are not really in dispute; that is to say, we are all agreed and firmly believe that pens and pineapples and bicycles exist independently of perceptions, while reputable scientists unanimously contend

that trees and mountains and the high seas existed long before there were human observers on this planet. I do not think that any modern philosopher seriously wishes to question these common beliefs and scientific contentions—and that is why I said just now that I would assume the metaphysical aspect of our problem could be profitably neglected. But what does worry the modern philosopher is whether it makes sense, logical sense, to apply a descriptive technique built up on sensory perception to objects held to be out of all relation, temporarily or permanently, to sensory perception; and he wonders, therefore, whether, from a strictly logical point of view, we are describing the situation properly at all. On the other hand, as the history of philosophy has shown, it is by no means an easy task to devise a 'proper' description of objects out of relation to perception; and that, no doubt, is why Locke failed to produce a logically satisfactory account, and why the phenomenologists produced an account which, despite its virtues, ignored what is essential in the situation. Locke, it is true, along with Descartes and Kant, confused the issue by making a speculative-factual analysis of a metaphysical kind, which involved propounding unverifiable, not to say absurd, propositions; but the phenomenologists, or at least the modern phenomenologists, although wisely concentrating on description, have devised a technique which not only prohibits them from taking account of the facts, but raises some peculiar and complicated logical difficulties as well. Moreover, the task which the phenomenologists, constantly and adroitly prodded by Professor H. H. Price, have felt themselves called upon to perform—namely, to disinfect the protases of their counterfactual conditionals from all physical-object expressions—is a task with self-stultifying implications; for the better it is performed, the more ambiguous and non-specific the description becomes.¹

How, then—and this is the problem we must now face—are we to devise a descriptive technique which does justice to the logic of the situation, i.e. avoids reducing existential categoricals, without remainder, to counterfactual conditionals, and yet takes account of the fact that we cannot properly describe any physical objects except in sensory terms? In other words, how can we devise a technique which deals *adequately* with physical objects when out of relation to perception, and similarly makes sense of what scientists tell us about the state of the world before there were human observers on our planet?

¹ Cf. Professor Price's discussion of Professor Ayer's *Philosophical Essays* in *The Philosophical Quarterly*, July, 1955, pp. 277-8.

Let us tackle this problem by easy stages. As a start, we may usefully glance at the way a modern phenomenalist would analyse two ordinary situations, one referring to the present and the other to the remote past.

(i) Suppose someone put the following question to a phenomenalist. "When my bicycle is locked inside my garage, with no one observing it, does it continue to exist?" To this the phenomenalist will presumably reply: "Yes, your bicycle does still exist in these circumstances, provided you will accept my analysis of 'exist', which is: if there were an observer present, he would be experiencing sense-data justifying the assertion 'This is a bicycle'". Our questioner might then retort: "I am not at all happy about your reduction of the factual categorical statement 'the bicycle still exists' to a hypothetical statement about what an observer would see if he were there. I agree that if an observer were there he would in fact have, as you would call it, bicyclic sense-data; but my question clearly implied that no observer would be present and I want to know whether, in these circumstances, the bicycle still exists." The reply to this naïve demand will be something like this: "The only way in which I can interpret your statement about the bicycle 'existing' when no one is in the garage is in terms of what *would be* sensed if someone *were* present; for the only meaning I can attach to 'exists', when this refers to physical objects, can be summed up in the phrase 'is or could be sensed'. Note too that neither you nor I nor anyone else can describe what a bicycle would look like if not being perceived by any sentient being, for the expression 'would look like' automatically implies 'would look like to somebody'." "But surely", our questioner hazards desperately, "you could just say something like 'When no observer is present in the locked garage, a bicycle could still be sensed there'?" "You can say it if you like", will be the remorseless reply, "but if you take the trouble to analyse the word 'bicycle' you will find that it is merely a *sign* used to refer to a fairly well-defined family of sense-data—hence you would be saying in effect that a certain family of sense-data, which we denominate 'bicycle', would be sensed if there were an observer present; and this is merely another version of my own analysis."

Now it is here that the worry arises, or that deadlock appears to be reached. For however convinced our questioner may be that something, namely his bicycle, continues to exist though not perceived, the phenomenalist clearly has reason on his side when he implies that none of us can get outside his skin and that it is sheer nonsense to try to describe physical objects like bicycles

except in terms of sensory perception. And although we may reject his talk about 'families of sense-data', we must still admit that we call an object a bicycle simply because it satisfies certain sensory specifications: it has this sort of appearance, these tactual qualities and performs this particular function which can be sensorily appreciated. Moreover, we can hardly disagree with the phenomenalist when he insists that the attempt to describe the 'inner constitution' of physical objects was just what led both Descartes and Locke astray. For we can neither verify nor falsify that the Cartesian or Lockian objects-in-themselves 'exist': there is no evidence, nor any possibility of obtaining evidence, that physical objects out of relation to perception consist of a set of so-called primary qualities—and this for the simple reason that we can never 'get at' objects out of all relation to perception and compare what they are like *then* with what they are like when being perceived: the whole idea is nonsensical. Now our questioner might be brought to see this, but might then say: "Is the bicycle then, the family of sense-data as you call it, still there, in the garage, when no human beings are there? Or, if I may put it more generally, do families of sense-data hang about the universe waiting to be sensed?" To answer this question in the affirmative leads to the postulation of Mill's unfortunate 'permanent possibilities of sensation' or of Russell's equally unfortunate 'unsensed sensibilia'; and both these explanations, as the more modern phenomenologists have recognized, represent a scarcely concealed relapse into a woolly metaphysics and raise more difficulties than they solve.¹ The modern phenomenalist would therefore regard both the particular and the general question just posed as pseudo-questions based on linguistic confusions; and he would hold firmly to his claim that his analysis *has* adequately described the unperceived bicycle in the garage and could be adapted to describe any unperceived object.

Nevertheless, we cannot be satisfied with this phenomenalist reduction of an existential categorical to a non-existential hypothetical: its implications, as more than one critic has pointed out, are logically indefensible.² Yet how *are* we to formulate our

¹ Cf. Mill: "Our sensations we carry with us wherever we go, and they never exist where we are not; but when we change our place we do not carry away with us the Permanent Possibilities of Sensation: they remain until we return, or arise and cease under conditions with which our presence has in general nothing to do." *An Examination of Sir William Hamilton's Philosophy*, Chap. XI.

² For a penetrating analysis and criticism of the phenomenalist position, cf. Mr. I. Berlin's article "Empirical Propositions and Hypothetical Statements", *Mind*, July, 1950.

descriptions about unperceived objects, and formulate them in such a way as to retain a categorical element, without laying ourselves open to the charge that we are talking nonsense? Before trying to answer this, let us move on to our second situation, which will serve to throw the problem into sharper focus.

(ii) Suppose a geologist, renowned for the accuracy of his inferences, informs us that about four million years ago there existed a landbridge, where there is now a strip of water, between the site of the present town of Malacca and the island of Sumatra; and suppose further the same geologist assures us that this landbridge disappeared into the sea just under four million years ago, i.e. well before human beings appeared on the planet. No human being therefore either saw, or could have seen, this landbridge, which nevertheless stayed *in situ* for some considerable period of years. Now although this example differs from that of the unseen bicycle in two significant ways—i.e. no one saw or could have seen the landbridge and its actuality can only be inferred from present geological evidence—the descriptive problem remains fundamentally the same; and the phenomenalist analysis would be: “If there had been an observer four million years ago, suitably stationed at the point we now call Malacca, he would have experienced sense-data justifying the assertion ‘This is a landbridge’”—or a similar analysis which attempted to ‘dissolve’ into sense-data both the observer and the physical landmarks in the if-clause.

The geologist, however, like the owner of the bicycle, would be unhappy about this analysis; for he firmly believes that four million years ago there was actually present, as a feature of the landscape, a long strip of brownish earth with stony outcroppings and the sea on either side which, on the basis of our present naming conventions, we would call a landbridge. To this, of course the phenomenalist would reply that whether you use the term ‘landbridge’, or the more complicated locution about the long strip of earth with the sea on either side, you are still referring to sense-data which some observer would have if suitably stationed—to these and nothing else. Moreover, we shall not, he would insist, get anywhere by trying to describe features of the landscape in non-sensory terms: this will merely result in complete unintelligibility. In short, we have got to describe this particular complex object, named for convenience ‘landbridge’, and held to have ‘existed’ when no humans were present on earth, in terms of the sensory experiences we believe actual living humans would have had; and we are thus com-

mitted to an analysis in the form of a hypothetical statement. So once again deadlock is reached; for we all do want, in company with the geologist, to make some sort of categorical statement which will do justice to the view that the earth and many of its present features, along with features no longer present like the Malacca-Sumatra landbridge, existed long before humans occupied this planet. But once again, how are we to do this? How are we to talk, in a *logically* satisfactory manner, about objects out of all relation to human perception *except* in terms of what hypothetical observers *would* perceive *if* they were present? Now the phenomenalist has rightly warned us to beware of talking about *Dinge-an-sich* with unknowable intrinsic qualities, since this will only lead us into absurdities; but we might at least consider, when trying to devise a suitable descriptive technique, the possibility of using some *words* with non-sensory implications—always provided this does not land us up in some metaphysical thicket. So let us now make a fresh start and see how far we get.

Suppose that two friendly disputants, A and B, are trying to elucidate this problem of describing, in a logically satisfactory manner, objects out of relation to perception. To start the ball rolling, A poses a deliberately naïve question about the remote past:

A. Can you doubt that millions of years ago there existed a world of physical objects, similar in so far as inanimate objects are concerned and largely similar in so far as the lower forms of life are concerned, to the world of objects with which we are acquainted to-day?

B. I am worried about your mention of 'physical objects'. Does not the very expression 'physical object' (a) have purely sensory implications, actual or possible, and (b) always denote what *we humans* have elected to call such? In other words, do we not in some sense *create* physical objects, first by discriminating and then by naming some particular feature or features of the world around us?

A. There is much point to your objection, and I see that my question lacked logical rigour. We do as it were break up the world, in the act of sensing it, into individual objects—that is what is meant by 'discriminating'; and it is just this discriminating and subsequent naming which, in a wholly non-mystical way, creates particular objects such as mountains, trees, clouds, rocks and so forth. I accept the term 'creates' simply because all the objects I have just named, and any others you can think of, would not *necessarily* be discriminated as independent particulars

(and hence 'objects') by some other sentient animal or being. Put in another way, we might say that these abstracted constituents of the world, which we call physical objects, are to some extent, though not wholly, *arbitrary* constituents. For example, where does this particular mountain 'really' begin and its adjacent valley 'really' end? Would other sentient beings discriminate the different strata in this piece of rock as we do?—and would the same piece of rock be differentiated from the hard earth in which it is embedded by a beetle?

B. Would you go so far as to say that, in sensing the world, we *change* it in some mysterious way?

A. Not at all. We do not change anything in the world either by sensing or discriminating or naming or describing; we merely, by doing these things, *take account* of the world in a way compatible with our peculiar sensory and conceptual apparatus.

B. I am glad you admit this. But there is another thing you said a moment ago which worried me, namely, that what we call physical objects are abstracted constituents of the world but not 'wholly arbitrary' constituents. I wonder what you meant by this. Are you perhaps assuming that there are, to put it crudely, constituents-as-such waiting to be discriminated, even though they might be and no doubt are discriminated in different ways by different types of sentient being, or not discriminated at all?

A. I see what you are getting at. When I talked just now of physical objects as 'abstracted constituents' of the world, and of these abstracted constituents being not 'wholly arbitrary', it looked as if I might be surreptitiously begging a question. For I seemed to imply that there were actually some *unknowable* constituents waiting to be made part and parcel of the specific constituents we 'construct' in discriminating or taking account of the world. The trouble, I suppose, centres in my use of the word 'constituent'; for it carries with it the implication that, before there were human or other sentient beings on earth, the world was already cut up into definite 'parts' which, in one way or another, could be discriminated. But this, I think, is a piece of illicit extrapolation from our present discriminatory processes. The only 'constituents' we can properly talk about are those which sensory perception, along with inferences based on sensory perception, enable us to regard as constituents; and we are now trying to describe the world at a time before any sensing or inferring had taken place.

B. Yes, that was really my point. 'Constituent' sounds a harmless enough word—and might even, in certain circumstances, be used in a way devoid of sensory implications (e.g. an electron is a constituent of an atom); but for your purposes, I think, it is unsatisfactory—unless of course you can put a new complexion on your talk about physical objects being 'not wholly arbitrary constituents' of the world.

A. I think I could do this if we switched our attention for the moment from features of the landscape to human beings. Suppose there were some kind of sentient being equipped only with visual sensory apparatus which functioned like an X-ray camera. To such a being our flesh-and-blood bodies would appear like so many skeletons; but in the case of every human being, *some* object—in this case a skeleton—would be discriminated. Again, if a number of human bodies came within range of a radar apparatus, a corresponding number of separate dots would appear on the screen. Considerations such as these could be said, I think, to indicate that human beings are not 'wholly arbitrary' constituents of the present world.

B. It may be that human beings are not wholly 'arbitrary' constituents of the world; but your arguments do nothing to establish this. In the first place, it is conceivable that a disembodied spirit (if this expression makes sense) might not discriminate human bodies, or any physical objects, at all; for such a 'spirit', therefore, a human body would be as non-existent as it is, so to speak, for a radio wave. Moreover, your appeal to radar gets you nowhere; for it is almost a commonplace now among physicists that any characteristics assigned to an object are inevitably dependent on the actual experimental process by which they are determined. In the second place, it is as difficult to describe human beings out of relation to perception as it is to describe trees and mountains. So if you wish to insist that human beings are not wholly arbitrary constituents of the world, this can only mean that they would be sensed, in some way or other, by some being equipped with some type of sensory apparatus. If there were a human being lying alone in a vast desert, we can no more describe what that human would 'look like' than what the desert would 'look like' *except* to a sentient observer of some kind. And this of course is merely the lesson which the phenomenologists have been driving home all along.

A. All right; let's return to the landbridge.

B. Very well. Suppose you claim—as I think you would have been inclined to claim a moment or two ago—that four million years ago there was a certain constituent between the site

of the present town of Malacca and the island of Sumatra which was different from the present constituent which we all agree in calling sea-water. Suppose further that you admit that if that past constituent were present to-day, we should call it a land-bridge. But what is this landbridge? From one point of view it is merely a strip of land continuous with the land in Malacca on the one side and Sumatra on the other; and we can only call it a *constituent part* of the world just because we humans actually discriminate and name the world in this particular way. Its being discriminated as a 'constituent', then, is a wholly arbitrary matter; we just do not know what other sentient beings would regard as constituent parts of the world, let alone what has a right, apart from all sentience, to be called a constituent part. The only meaning, therefore, which I can attach to your phrase 'not wholly arbitrary' is this: that in order for us humans to abstract constituents (which we call physical objects), there must be, and for a long time have been, some 'objective whole' from which we abstract them. Would you agree that this was the actual cash value of your phrase?

A. Yes, I think I would agree—and this implies that I have been making a metaphysical assumption. On the other hand, whether or not the expression 'objective whole' is fortunate, it is the minimal metaphysical assumption which we all have to make if we are to accept the view that the world and the universe, however they are properly *described*, did exist before there were human observers on the planet Earth.

B. I think this assumption is so well grounded, and requires such a woolly metaphysic to undermine it, that we can accept it without hesitation. And in any event we do not want to get bogged down in some metaphysical morass. But I must still point out that we have not yet solved our *descriptive* problem.

A. I think I have realized this all along. But—to continue with the landbridge—we must surely admit there was *something* between Malacca and Sumatra?

B. Certainly 'something' was there—that is implicit in our whole common-sense and realist approach; but what was that 'something', or rather, how are we going to describe it?

A. Let me retrace my steps a little. I stressed earlier, by implication at least, that whatever there was between Malacca and Sumatra—or however it should be described—our talking about it does not alter it in any way, any more than our talking about what lies there now, and calling it a strip of sea-water, alters this in any way.

B. Go on.

A. Well, I think now that, instead of talking about 'constituents', which raises the difficulties you have pointed out, we might refer to the world of four million years ago as being pervaded by varying structures or patterns, irrespective of how these structures and patterns would have been discriminated by sentient beings. This would presumably be a fair inference from scientific discoveries in general.

B. Yes, I think this is a fair enough inference and that it takes us a little further towards our goal; but I am not altogether happy about your use of the terms 'structures' and 'patterns' since these are instinct with sensory implications. However, let us see how you propose to incorporate this new piece of terminology into your descriptive statement about the landbridge.

A. Well, I would say something like this. Four million years ago there was a world pervaded by certain structures or patterns such that, if an observer had been present and suitably stationed, he would have actually discriminated a complex object, which he would have called a landbridge, between the place now called Malacca and the island now called Sumatra.

B. I still don't like 'structures or patterns' but I think we have now got our vital clue to descriptive propriety—and it is to devise a categorical in non-sensory terms and couple it with a hypothetical in sensory terms. If I am correct, then our remaining problem is to find the right sort of categorical.

A. What do you suggest then?

B. Let us go back even further in time than the supposed Malacca-Sumatra landbridge. Suppose we ask an astrophysicist, Mr. Fred Hoyle for instance, to tell us about the cosmological event primarily responsible for the formation of our earth. He would reply as follows: "There was once [i.e. about three thousand five hundred million years ago] a star moving around the sun that disintegrated with extreme violence. So great was the explosion that all the remnants were blown a long way from the sun into space with the exception of a tiny wisp of gas out of which our planets have condensed." (*The Nature of the Universe*, p. 72). Notice that nearly every word in the passage quoted is a word only intelligible in the context of sensory experience; and that Hoyle's description therefore raises the same difficulties as did the geologist's description of the Malacca-Sumatra landbridge. How, then, are we to make Hoyle's description more logically satisfactory? By translating it into something like this: "About three thousand five hundred million years ago¹

¹ Time-descriptions raise logical problems too. I tried to deal with this particular one in my article "The Concept of Time", *The Philosophical Quarterly*, Jan., 1955, p. 17.

there was a state of affairs such that, if a human observer had been present and suitably stationed, he would have justifiably talked about a binary star-system exploding, with one member (the supernova) receding rapidly into space and leaving behind a small wisp of gas which then started to rotate around the other member of the binary system." We can deal with the geologist's statement on similar lines: "About four million years ago there was a state of affairs such that, if a human observer had been present and suitably stationed, he would have justifiably talked about a landbridge connecting the present site of Malacca and the island we now call Sumatra."

A. So our existential categorical—the proposition which is to ensure that our analysis ties up with both common sense and scientific discovery or inference—should, you suggest, take the form of 'There was a state of affairs such that', and be followed by the appropriate counterfactual conditional which fills in the actual content and thus makes the categorical precise and specific?

B. Yes, that is my solution, offered with some hesitation but also in the belief that it evades the logical difficulty involved in trying to replace a directly or indirectly verifiable existential categorical by an unverifiable unfulfilled conditional and nothing else. Moreover, the same formula, with appropriate variations, covers all cases of present unperceived objects, such as that of the bicycle locked inside the garage—e.g. "There is a state of affairs such that, if an observer were now in your garage and suitably oriented, he would be justified in saying 'I see a bicycle'." Notice that the expression 'state of affairs' is one which (a) can be and here is used without sensory implications, and (b) does not assume, in advance of human science, that there are *particular* constituents waiting to be discriminated or *particular* sensibilia waiting to be sensed. It is beyond our powers—whether we be laymen or scientists—to state what *are* the actual constituents of the universe independently of human discrimination or humanly conducted scientific experiments; hence 'constituents', as I have already emphasized, is in this context a question-begging term. On the other hand, the expression 'state of affairs' does not carry with it any mysterious metaphysical implications and is compatible with the world or the universe 'appearing' to non-human cosmic observers, if there are any, in ways different from what it would or does appear to human observers.

A. Although I agree with your analysis, it does seem that our joint labours have produced a rather small mouse!

B. True, but smallness is not necessarily a sign of triviality. We have agreed on a translation or analysis which (a) retains the categorical element which is fundamental to both common sense and science, (b) concedes to the phenomenologists that physical objects, from stars to bicycles, can only be described in normal sensory terms, (c) avoids the logical complications of the pure unfulfilled conditional, and (d) does not beg the question as to what constituted physical objects before human beings with sensory and conceptual powers were present to discriminate, name and describe the world and the universe. Moreover, the problem of verification is left on a completely common-sense basis. If someone claims there *is* a bicycle in his locked garage, the analysis reiterates this claim and verification or falsification occurs when the garage is opened up; and if someone claims there *was* a landbridge four million years ago between Malacca and Sumatra, the analysis reiterates this claim too and verification or falsification depends on the expert acceptance or rejection of the induction from the present geological evidence.

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CRITICAL NOTICE

FORMAL LOGIC. By A. N. Prior. Oxford University Press, 1955. ix, 329 p. £2. 19s. 3d. (Australian).

This book is marked by a wholly admirable eclecticism. In no other comparable treatise on logic, so far as I know, are so many alternative systems discussed. Not only do we find descriptions of the usual PM systems, the Lewis modal systems and the intuitionist system of Heyting, but we also find accounts of some of the work of such Polish logicians as Lesniewski, Lukasiewicz, Suszko, Slupecki, Wajsberg, Sobocinski and Tarski. Some of this work is thus made easily accessible for the first time. Prior has often had to dig it out of little-known journals or even sometimes to reconstruct papers from hints given in reviews in the *Journal of Symbolic Logic*. Though, as I said, the book is eclectic, it nevertheless bears the stamp of Prior's powerful and original personality. It also contains, of course, original solutions of Prior's own to certain problems.

Unlike Strawson's *Introduction to Logical Theory*, this book really is an introduction to formal logic (symbolic logic). Strawson's important book is not an introduction so much as a blackballing: a timely warning to those who have already studied the subject and have misapplied it to metaphysical purposes. Where Strawson introduces the reader to the propositional and predicate calculi he does it so unsympathetically, with so little regard to the beauty of a logical system or to the intellectual interest of the metalogical questions that can be asked about it (indeed there is no discussion of metalogical questions) that the reader who knew nothing of the subject might wonder, from Strawson's account, why anyone should ever bother to study it. No one could get that impression from Prior's book. Here he will find himself introduced to symbolic logic itself, and if after reading Prior he does not come away thinking that it is a most enjoyable and rewarding discipline then I think there is no hope for him.

Why, apart from its intrinsic beauty, is symbolic logic a rewarding subject? Here I may perhaps be allowed to express a personal opinion. I am not one of those who think of symbolic logic as metaphysically useful: I do not think that we can help ourselves to answer metaphysical questions by translating them into some artificial language got by interpreting some logical calculus. I rather think that Prior *does* think of symbolic logic as a handmaid of metaphysics, but it is fair to say that this

motive does not obtrude itself in his book. (Though see p. 201 for an interesting application to the critique of arguments for the existence of God.) My main reason for thinking that symbolic logic is important is that it is indispensable for a proper understanding of mathematical systems.

(1) Symbolic logic may help us to make pure mathematics itself more rigorous. For example, notions of limit, convergence, uniform convergence and so on become clearer when expressed in terms of quantification theory. Again, consider this example. In the propositional calculus if we can prove a thesis '(G)' from an assumed thesis '(F)' we can prove from the axioms alone the thesis 'if (F) then (G)'. On the other hand, in the predicate calculus this rule of conditionalisation is valid only under a certain restriction. (See Kleene, *Introduction to Metamathematics*, p. 97.) This leads us to enquire into the conditions under which the rule of conditionalisation is valid in ordinary mathematics. Normally mathematicians use it light-heartedly: they assume '(F)', deduce '(G)', and then say that they have proved 'if (F) then (G)'. But consider this argument: Assume

$f(x) = 0$. Differentiating we get $\frac{d}{dx}f(x) = 0$. So if $f(x) = 0$,

$\frac{d}{dx}f(x) = 0$, an obviously false conclusion. What has gone wrong

here? A student who is trained in formal logic would soon be able to sort this one out, and formulate conditions under which it is safe to use the rule of conditionalisation.

(2) More important than the above use of symbolic logic *within* mathematics is the use of logical systems as examples of mathematics. For mathematics as it is ordinarily done is a thing with no holds barred: to solve a problem in number theory, for example, it is quite in order to use the methods of analysis. Any method that helps to solve a problem can be used, no matter how different the concepts involved in it appear to be from the concepts of the problem. It is thus extremely difficult to discuss the structure of mathematics as it is in fact done. What we need is a number of very simple systems which stand to mathematics proper as Wittgenstein's 'language games' stand to ordinary language. When we grasp the metalogical properties of these systems and view these systems *as a whole* we get an insight into the nature of formal systems in general. We find systems that are weakly but not strongly complete, systems that are strongly complete, systems with decision procedures, systems without decision procedures, systems which admit of 'creative definitions'

(Lesniewski) and systems that do not, and so on. Again, of course, in elementary number theory we have the famous Gödel theorem. After studying these fragments of mathematics we get an insight into the whole untidy system of mathematics itself. This insight can come only from comparing the various differing metalogical properties of various systems: it cannot come from studying intensively one particular system or group of systems. And this shows us the particular merit of Prior's book. Prior does take us on a world tour and we see all sorts of scenery. Of course Prior is interested in the logical interpretations of the systems, but the systems are interesting apart from this. Moreover, it is a mistake to think of, say, modal logic as exclusively logic. It can equally be given a topological interpretation. This fact is important, because some philosophers may be tempted to dismiss some systems because they do not find interest in the logical interpretation of them. But the systems may have interesting formal properties and be illuminating quite independently of their interpretation, and moreover they may well be susceptible of non-logical interpretations which may turn out to be of great importance.

The book is divided into three parts. Part I is on the classical propositional and predicate calculi. Chapter I deals with the notion of a truth function, with the interdefinability of truth functions, and with tautologies. In Chapter 2 we get the PM axioms for the propositional calculus, Lukasiewicz's axioms for a C—N system, Nicod's and Meredith's single axiom systems, and the axiomless systems of Suszko and Popper. The Hilbert and Ackermann completeness proof for the propositional calculus is next given, and it is shown that the propositional calculus is not only weakly complete (all tautologies can be proved within it) but that it is also strongly complete: if any proposition which is neither an axiom nor a provable theorem is added to the axioms the system becomes inconsistent. I think that here Prior could have made the relation between weak and strong completeness a little clearer. The relation can be brought out as follows. Weak completeness is relative to an interpretation: that is, a system is said to be weakly complete if it serves some understood purpose, i.e. of proving all propositions of a certain class. It can in general be made complete for a wider purpose by adding on further axioms. Sometimes, however, a system cannot be made stronger in this way without becoming so strong as to enable us to prove all propositions whatever, which means that the system is inconsistent. We can say that if a system is so strong that it cannot be made any stronger while still remaining con-

sistent then the system is strong enough for any legitimate purpose. The only purpose for which it could be made stronger would be for proving all propositions whatever, which no one wants to do.

In Chapter 3 we find the Tarski-Bernays axioms for the implicational fragment of the propositional calculus, which with the addition of a further axiom become Wajsberg's axioms for a C—0 system. Quine's completeness proof for the C—0 system is then sketched. So also is the rather similar proof (from which Quine's took its origin) by Wajsberg for the purely implicational calculus. Next comes a description of Lesniewski's 'protothetic', that is, the propositional calculus with variable truth-operators, which has been reduced to a single axiom by Lukasiewicz, and to an even shorter axiom by Meredith. In Chapter 4 we find the theory of quantification, the restricted predicate calculus. There is also a discussion of the propositional calculus enriched by quantification over propositional variables, and of the enriched protothetic of Lesniewski which is got in this way. This leads on to an extremely interesting criticism of Lesniewski's and Lukasiewicz's account of definitions as axiomatic assertions of equivalence. Prior comes down against this view, and argues for the PM account of definitions as abbreviatory devices. Prior has recently informed me that he has changed his views on this matter and that he has come round to a point of view more sympathetic to Lesniewski. However, this does not impair the value of the discussion in the book which provides a very good introduction to some of the issues involved in this controversy.

Part II is mainly concerned with the Aristotelian syllogistic and modifications thereof. There is a great deal of historical material which would be of interest to those who like that sort of thing, but the subject appears to me to be on the whole a messy one. It is also of little fundamental value and there is not much in this part of the book that is necessary for understanding Part III, apart from pp. 178-182, which deal with the theory of descriptions and which are essential. Such things as the controversy between Sir William Hamilton and de Morgan on the quantification of the predicate would have been better left in the obscurity from which Prior has rescued them. The interest of Part II is mainly historical and the information contained in it could perhaps form part of a separate work on the history of logic. I might mention that there is a wealth of interesting historical material scattered through the whole of the book, but Part II seems to me to be the only part where the history gets in the way of the logic.

In Part III we have first of all a chapter on Modal Logic. The various Lewis systems are given, and also the axiomatisations of Gödel, Feys and Von Wright. Prior also gives a system of his own which is said to be equivalent to the Lewis system S5 and which is got by adding to the ordinary assertoric calculus four rules which are analogous to the rules for quantifiers in the predicate calculus. On the subject of the Lewis systems S4 and S5 it is interesting to ask whether the characteristic theses of these systems, namely 'if possibly possibly p then possibly p ' and 'if possibly p then necessarily possibly p ', are as intuitively doubtful as Prior seems to think they are. It seems to me that if we interpret 'possibly' and 'necessarily' in the sense of *logical* possibility and *logical* necessity, then these theses express obvious truths. For if we define 'logically necessary' as 'such as can be discovered to be true by a conceptual investigation alone' and 'logically possible that . . .' as 'not logically necessary that not . . .', i.e. as 'not such as can be discovered to be false by a conceptual investigation alone', then the characteristic theses of S4 and S5 turn out to be true. For that a proposition is possibly true can be seen by an investigation of its form alone: for example if we understand the sentence 'the cat is on the mat' we understand that the proposition it expresses can be true or false according to the facts of the case. If anyone thought that 'the cat on the mat' expressed a proposition of a different sort, namely a necessary or impossible one, we should have to say that he did not understand it, or alternatively that he did not understand the meaning of 'necessary' or 'impossible'. It can be seen by a conceptual investigation alone that we cannot tell by a conceptual investigation alone whether or not the cat is on the mat. So, in the general case, we have the S5 thesis 'if possibly p then necessarily possibly p '. From this we can deduce the S4 thesis 'if possibly possibly p then possibly p '. (The system S4 is included in S5). Similar intuitive considerations, by the way, show that the Lewis S7 and S8 theses 'possibly possibly p ' and 'necessarily possibly p ' are intuitively not just doubtful but definitely false. For suppose p is a proposition of the form ' q and not q '. It can be seen from the form of this alone that it cannot be true. A conceptual investigation is all that is necessary to assure us of this. So it is not the case that possibly possibly both q and not q . Hence 'possibly possibly p ' cannot be a law. Similar considerations of course also rule out 'necessarily possibly possibly p '. Two other queer theses discussed by Prior are 'if necessarily possibly p then necessarily p ' and 'if possibly p then possibly necessarily p '. These are ruled out by intuitive

considerations of the sort I have just adumbrated. This is not surprising, because W. T. Parry has shown that they imply 'if p then necessarily p ', which causes modal distinctions to collapse. (Since we have 'if necessarily p then p ', anyway, the above thesis makes ' p ' and 'necessarily p ' equivalent to one another.) In this connection I would like to draw attention to the similarity between the fallacy of inferring 'possibly necessarily p ' from 'possibly p ' and the fallacy of inferring 'possibly both p and q ' from 'possibly p and possibly q '. For to deduce 'possibly necessarily p ' from 'possibly p ' is to deduce 'possibly true and conceptual', i.e. 'possibly both true and true-and-conceptual', from 'possibly true and possibly true-and-conceptual'. (There are two ways of being true. One is just being true: the other is being necessarily true.)

Part III, Chapter 1, continues with the introduction of modal operators into quantification theory and a section on deontic logic, which includes a consideration of some interesting paradoxes into which we can easily be tempted if we introduce modal operators into deontic logic and accept a rather plausible principle. In particular we can easily be led into asserting the 'principle of continuous moral rectitude', as Professor G. E. Hughes has called it, that either nothing is obligatory or everything that we do is obligatory.

Part III, Chapter 2, is on three-valued and intuitionist logic. Certain three-valued matrices for implication and negation are given. In contrast to the case with two-valued logic, these two truth functions do not suffice to define all possible three-valued truth functions. That is, the three-valued system based on these two truth functions is not 'functionally complete'. However, Slupecki showed that the system could be made functionally complete if a third fundamental truth function Tp is added such that Tp takes the intermediate truth value no matter what truth value p takes. Wajsberg's axioms for the system based on implication and negation alone are given. These give us a system that is only weakly complete. Weak completeness is a semantic notion, not a purely syntactical one, but Prior points out an interesting fact. This is that we can give a purely syntactical definition of the sort of weak completeness possessed by the Wajsberg axioms. If we add on two axioms (due to Slupecki) containing the operator T we get a system that is strongly complete. Hence we can describe the completeness of the Wajsberg axioms by saying that they yield all formulae which can be consistently combined with the Slupecki axioms in T . Whether such a syntactic characterisation of the weak

completeness of a set of axioms can be carried out in other cases is an interesting topic for investigation. There follows a discussion of Aristotle's 'sea-battle tomorrow' in which Prior puts forward a set of three-valued matrices due to Lukasiewicz. These can be interpreted as giving a logic of future contingents containing neuter propositions in Aristotle's sense. Certain modifications to Aristotle's doctrine turn out to be necessary to preserve consistency. In particular ' p or not p ' has to be given the neuter value when p is neuter. I suspect that Prior thinks that his discussion here has metaphysical relevance. His language is at any rate misleading when he says on p. 241 'if there really are propositions which are neither true nor false but just "potentially either" . . .'. I would prefer to say 'if we adopt a language with the conventions that . . .'. Chapter 2 concludes with a discussion of Intuitionist logic.

Chapter 3 of this Part gives us an extremely able, lucid, and altogether useful summary of that difficult subject, the PM theory of classes and relations in extension. Possibly some of §1 could have been omitted. The difficulties into which we get if we admit non-extensional functions to PM could surely have been most easily bypassed by saying that no such functions as modal ones need be introduced in the first place. There is a discussion of the Logical Paradoxes, in which Prior, so far as I can see from his slightly metaphorical language, boils the trouble down to some modified form of the vicious circle principle. He interprets Ryle's treatment of 'heterological' (*Analysis*, January, 1951) in this sort of way but says that Ryle's treatment 'is marred by being embedded with a quite different and not at all plausible explanation of the fallacy'. (p. 289, footnote.) Prior does not here, however, it seems to me, lay his finger on what, with the heterological paradox and the liar paradox at any rate, Ryle regards as the fundamental fallacy. This is the admission of descriptive phrases for which it is in principle impossible to give an implementation by a 'namely' rider. See Ryle's article at p. 67, near the bottom. Furthermore, I am doubtful whether Prior's solution of the paradoxes is not so sweeping as to rule out such apparently respectable things in classical mathematics as the proof of the theorem of the least upper bound. The chapter closes with a discussion of Lesniewski's systems of ontology and mereology. These systems are intrinsically interesting but should perhaps have been treated in a different chapter. The 'classes' in mereology are not really classes at all but are concrete aggregates. For this reason a class, in Lesniewski's sense, possesses no cardinal number. It may be, say, both 10 (piles of

paper), 1000 (sheets of paper) and so many million (molecules). Ontology is extremely interesting as an alternative way of securing 'individual reference' instead of Russell's apparatus of 'logical proper names' (or variables for such) plus descriptions. It also lends itself extremely well to the discussion of certain points about definitions.

I shall now pass on to some remarks about the general arrangement of the book. I think it would have been better if three-valued and intuitionist propositional logic had been treated together in Part I, thus regarding 2-valued propositional logic as a special case of n -valued and intuitionist logic. (Intuitionist logic is in effect infinite-valued.) Then the general metalogical problems of consistency, independence and completeness could have been treated together. As it is, some of these are discussed in Part I, Chapters 2 and 3, and some in Part III, Chapter 2. Furthermore, the important question of whether a calculus has a decision procedure is not raised in its general form. It is not mentioned, for example, that the predicate calculus has no decision procedure. The student might easily be misled by the examples of completeness proofs which are given. All these (Hilbert and Ackermann's, Quine's and Wajsberg's) are for various forms of the propositional calculus, and consist in turning a decision procedure, so to speak, 'back to front'. The reader might therefore wonder how completeness can be proved for the restricted predicate calculus, for which there is no general decision procedure. For this reason it would have been useful if Prior could have given a short description of Gödel's completeness proof for the restricted predicate calculus.

There is a very brief description of Gödel's incompleteness theorem for elementary number theory on pp. 70-71. This follows Findlay's note in *Mind* for 1942, and is in my opinion extremely misleading. Findlay's ordinary language equivalent of Gödel's unprovable sentence contains in it the word 'statement'. Now does Findlay mean 'statement' or 'sentence'? If it is 'statement' then Findlay's Gödelian sentence is meaningless for the same reason as is the paradox of the liar or of heterologicality: it contains a descriptive phrase which in principle cannot be given a 'namely' implementation. (Cf. Ryle's article on 'Heterologicality', already referred to.) If by 'statement' Findlay means 'sentence' then he cannot mean by 'we cannot prove' what we intuitively mean by it: he must mean something which can be defined only by recursive methods and with all the metamathematical paraphernalia that his treatment gives the appearance of dodging.

On p. 40 Prior uses the words 'what we require is a new rule of inference'. It should have been made clear that the substitutivity of equivalents is not a new rule of inference (though in practice we treat it as such) but a metalogical theorem. On p. 84 it should have been said more explicitly that the domain of individuals for which we interpret the predicate calculus is a non-empty domain. After 'truth-functions' on p. 191, line 7, there should be the qualification 'in a two-valued logic'. I can make no sense of the footnote on p. 268, though here of course Prior is trying to reproduce a passage in PM which is itself very confused. P. 271, line 2, would be less ambiguous if 'none' were replaced by 'no members'. On page 280, two lines from the bottom, 'in some cases' is a bit weak. I also note some misprints: On p. 17 formula (viii) should be CKOsCqAOs, and formula (ix) should be COCqs. On p. 18, line 14, for 'COs' read 'CoCqs'. On p. 34, line 9, for 'from' read 'form'. On p. 134, line 10, for 'opposite directions' read 'the same direction'. On p. 224, line 19, for '(NPc)' read '(NPa)'. On p. 244, line 15, for 'CNpp' read 'CNpq'. On p. 268, second line from the bottom, there is a bracket after ' $\pi 1$ ' that should not be there. On p. 309, in postulate set 8.4, the reference after 'Lukasiewicz' ought to be 8.1, not 7.1, and the formula after 'Meredith' ought to have another 'C' after the 's'. On p. 311, postulate set 10.4, in the axiom the C at the beginning of the third line should be removed from there and placed before the second 'K' of the first line.

In conclusion I should like to say that I have had the pleasure of reading some lecture notes of Prior's, composed since this book was written, in which a great many improvements have been carried out. They bring together the material on propositional logic, and relegate the syllogistic matter to an appendix. The whole treatment is more concise and there is some interesting new material. I hope that this book will be rapidly sold out, as it deserves to be, so that these improvements can be incorporated in a new edition. But even without these improvements the book is outstanding in its field.

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REVIEWS

KANT. By S. Körner. Penguin Books, 1955. 230 p. 2s. 6d. (U.K.).

KANT'S FIRST CRITIQUE. By H. W. Cassirer. London, Allen and Unwin, 1954. 367 p. 30s. (U.K.).

The authors of these two books share the conviction that one can engage in a study of Kant without ever indulging in profitless antiquarianism or neglecting the pursuit of genuine philosophical enquiry. Thus Körner holds that "so far from leading to the graveyard of philosophical problems, an introduction to Kant's philosophy may hope to lead to questions which are still alive and important" (p. 16); and Cassirer writes, more contentiously, "the errors committed by the great philosophers are, generally speaking, not only more interesting but more fruitful than the truths propounded by lesser men". (p. 19). Each is concerned, in his own way, with the vindication of philosophical scholarship.

Professor Körner has given us a remarkably compact and reliable introduction to Kant's Critical Philosophy. With commendable brevity and clarity he not only presents us with a fairly detailed account of the arguments of the *Critique of Pure Reason*, but introduces us to the central theses of the *Critique of Practical Reason*, the *Groundwork of the Metaphysic of Morals*, and the *Critique of Judgement*, while touching on that less known work *Religion within the Limits of Pure Reason*. His exposition is always able and sympathetic, making it evident that he has successfully 'thought himself into' Kant's system and has communicated the internal understanding so acquired. Seldom critical, he has sought rather to translate Kant's somewhat forbidding terminology into the language of present-day philosophical discussion. At times he gives Kant an almost contemporary ring. For example, of the faculties bogy he writes: "Kant's terminology of faculties corresponds to distinctions between different types of experience and consequently to different types of proposition and types of concept and thus, in the end—if we are articulate enough—to different types of linguistic expression and usage. There are occasions when it is advantage than differences in that about which we are talking. The present

is not, as I believe, one of these occasions. The modern linguistic philosopher has not always the patience to learn to understand Kant's terminology. If we are to think of him and if we wish him not to miss what Kant has to say, we might perhaps explain to him the notion of practical reason by saying that for Kant the logical grammar of the term 'duty' is different from the grammar of 'desire' and 'impulse'; while yet the phrase 'conflict of duty with desire' is for him just as correct as the phrase 'conflict of one desire with another.'" (p. 129f.). Such translations into the concepts and idioms of contemporary philosophy are always illuminating, even if Kantian devotees may doubt their propriety.

Körner brings out the strong topical interest of Kant's thought when he shows its relevance to some of the issues that still exercise philosophers. Thus he devotes several sections to the discussion of such problems as the following: Are Kant's synthetic *a priori* judgements an effective answer to the logical positivists? Are mathematical propositions analytic or synthetic? How is our physical-object language possible—that is, does it involve the operation of certain applicable, but not abstracted, principles or rules like Kant's *a priori* principles? Are presumptively moral judgements used merely for the expression and evocation of attitudes or are they absolutely objective in a way that excludes the possibility of alternative moral codes? Can teleological statements be reduced to non-teleological ones, or should we regard them both, as Kant does, simply as methodological *maxims* for use in certain types of explanatory theory? Very sensibly Körner does not exhort the reader to close his Russell, Moore, Stevenson and so on, but rather recommends him to open, or re-open, as the case may be, Kant's philosophical writings.

But the book is, for the most part, expository; and one or two of its highlights deserve special mention. Particularly useful as a guide through the intricacies of some of Kant's most difficult argument is Körner's "caricature" of Kant's deduction of the categories: "To derive the category embodied in the logical form, take any objective empirical judgement of this form and find the concept, the omission of whose application will turn the given judgement into a purely perceptual one." Even more briefly: "Objective empirical judgement $\ominus$ minus category X = a perceptual judgement." This may indeed be a "crude exegetical game", as Körner himself admits, but he has played it effectively. Again, the function and importance of the schemata is made clear by distinguishing 'referential' and 'non-referential'

rules governing the use of a concept (compare rules of semantics and 'logical grammar' respectively) and likening the schemata to referential rules linking concepts to perception.

If his book suffers from occasional inadequacies, Körner may, I think, plead circumstances of size and scope in mitigation. Perhaps something more *could* have been said, for example, about the Copernician Hypothesis, the function and significance of the threefold synthesis of the Imagination, the relevance of the second edition restatements of the schemata and proofs of the Principles of Understanding to the Refutation of Idealism, and so on; but, on the whole, Kant's theses have been summarised as systematically and authoritatively as could be expected in an *introduction*. And it is as such that this book will commend itself to student and philosophical amateur alike.

There are, by the way, some unimportant but disfiguring misprints—pages 157, 188, 192 and 200—and a rather more serious mistake on page 89: in the passage reading, "postulates of empirical thought . . . correspond . . . to the Categories of relation", substitute 'modality' for 'relation'.

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The problem of giving a brief, but accurate and informative, description of Cassirer's book has given me some concern: the claim on the dust-jacket is false (the book does not deal 'fully' with the 'whole' of the first *Critique*); and the author's own description is vacuous (exactly what should be expected of 'an appraisal of the permanent significance of Kant's *Critique of Pure Reason*'?). It is, I suppose, a commentary of sorts—a kind of study which will supplement rather than compete with the detailed exegesis and textual criticism found in the commentaries of Kemp Smith and Paton. For, although a lot of solid and painstaking thinking has gone into this work (Cassirer's scholarship is unquestioned), it by no means conforms to the usual requirements of a commentary, e.g. complete analysis of the text and explication of all its obscurities, with careful division of the argument into its various stages, appropriate use of chapter and section headings for the guidance of the reader and a minimum of talk about what the author *should* have said but didn't. What Cassirer offers is a critical study of the *Critique* characterised by its incompleteness (several long and important sections are omitted—some even without mention), its austerity in presentation and organisation (there are few helpful signposts), and the unapologetic expression of Cassirer's own philosophical opinions.

In his preface Cassirer professes a certain scepticism concerning the nature of philosophical thinking: it does not seem to him "to be the sort of thing that lends itself to the yielding of objective truth" (p. 17). And this attitude is carried over into his view of much philosophical scholarship (his own included): "It would be a good thing", he feels, "if all those setting out to expound the philosophical system of a great man . . . made it clear to themselves from the outset that their finished product would invariably contain distortions introduced not only by the special bent of their interest and the opinions they themselves happen to hold but also by wholly subjective factors such as their temperament and psychological make-up." (p. 18). He confesses that he has allowed himself to be largely influenced by his "personal agreement or disagreement with his [Kant's] point of view, and even by purely personal reactions produced in me either by his own observations or by opinions concerning his doctrine which are prevalent among his foremost expositors." (p. 12). This will explain, even if it does not justify, much of his procedure. In general he is interested in those features of the *Critique* which he hopes will assist him to clarify his own ideas about epistemology. Accordingly, its main tenets are subjected to criticism, restatement and subsequent modification, the intention throughout being to arrive at some positive suggestions for a realist theory of knowledge constructed along the lines of a sort of Kantian phenomenalism.

It is only with reluctance that Cassirer devotes his first chapter to a discussion of the Transcendental Aesthetic, for he claims to find most of it unconvincing and obsolete. On the other hand, he is sympathetic towards Kant's thesis that "all sensible existents can, in the last analysis, be resolved into presentations" (p. 43); this, one gathers, is the sort of thing he too would say. But he holds Kant to be wrong in his estimate of the part played by the *a priori* in perceptual knowledge, arguing, in a curiously dogmatic way, that 'we must insist that knowledge can in no circumstances involve an element of making: it is contrary to the very *idea* [my italics] of knowledge that it should have the effect of transforming that which was to become known" (p. 39). This quasi-ontological sort of argument is a recurrent one (see further his allegations of 'unwarranted subjectivism and idealism', pp. 61, 65, 75f., 96, 144f., 356) and is, by the way, directed against what Kant called the "main purpose of this critique of pure speculative reason", viz., that "reason has insight only into that which it produces after a plan of its own" (B xiii). What is curious about all this is that, after arguing that every-

thing *a priori* is subjective and that anything into which an element of subjectivity enters is not 'proper' knowledge, he later shows that Kant has quite definite criteria by means of which to differentiate clearly between the subjective and objective. What then does Cassirer mean by 'subjective'?

In Chapter II Cassirer follows Kant in making a logical distinction between two elements in human knowledge, intuition and understanding, or, as he would prefer, sensation and interpretation, and he readily admits that at the level of *interpretation* there is at least a *prima facie* case for the operation of *a priori* principles. The argument of the Metaphysical Deduction, however, is dismissed summarily as untenable and vicious in principle because it proceeds from "bare forms of thought, such as are studied by formal logic" (p. 58).

It is primarily because the language of the second edition Transcendental Deduction is more appropriate to an epistemological enquiry than the psychological language of the original version that Cassirer disregards the latter. In an interesting passage (pp. 69-70) he sketches an analogy between the *a priori* principles of understanding and the function of a language: language, he says, interprets the sense-given so as to guarantee communication and objectivity. That is, it does the sort of thing in a general way which Kant's categories were supposed to do in a particular way and, like them, its warrant extends only as far as the boundaries of sense-experience: with Kant he cautions us against substituting 'pure categories' for the 'schematised' ones which are involved in our consciousness of things.

A short chapter (IV) on the *a priori* principles of Understanding takes for granted the validity of the distinction between analytic and synthetic propositions and equally without a qualm moves on to a consideration of Kant's synthetic *a priori* principles. These are regarded as second-order propositions which specify the ultimate conditions of factual truth (a fair characterisation of Kant's doctrine, I should think).

Kant's validation of the concept of quantity (Axioms of Intuition) is made the occasion for a lengthy treatment in Chapter V of the problem of objective reference, i.e., how it is that we bridge the gap between private impressions and real (public) objects. In keeping with his realist sympathies, Cassirer tries to reconstruct a doctrine of perceptual consciousness in which the principles of the understanding, far from interfering with the given, recognise it as what it really is.

Despite Kant's unequivocal declaration that the Analogies are purely regulative principles, that is, procedural rules laying

down general conditions for the discovery of objective matters of fact, in Chapters VI and VII Cassirer accuses Kant of treating the principles of Substance and Causality as themselves factual propositions. Allegations of this sort need to be supported by carefully documented evidence—especially when there is a strong presumption to the contrary—but here, as occasionally elsewhere, Cassirer tends to substitute mere assertions for arguments. On the other hand, his constructive suggestions for the understanding of Kant's doctrine of substance are well worth while. By contrasting *substantia phaenomenon* with the absolute substance of metaphysics, he clears the way for treating phenomenal substance as more or less the equivalent of *thinghood*. And this notion of thinghood, he further suggests, is implicit in the way we talk about alteration, as distinct from mere change. Compare: "In all appearances the permanent is the object itself, that is, substance as phenomenon" (B 227); and "we can say . . . that only the permanent (substance) is altered, and that the transitory suffers no alteration but only a *change*" (B 230). By means of what looks like a mere change in terminology, Cassirer instructs us on a difficult point in Kantian doctrine and clarifies our everyday use of 'substance', 'object' and 'thing'.

That the fidelity of Cassirer's picture of Kant is sometimes suspect must already be apparent; but his treatment of Kant's doctrine of the thing-in-itself is crude to the point of positive distortion. What he does, in effect, is to attribute to Kant a view which he would repudiate and then use Kant's own arguments to discredit it. And the paradoxical result is that, although he holds that all of Kant's statements concerning the nature of things-in-themselves are "found to be wholly indefensible", he finishes up by endorsing most of what Kant actually said on the topic. Thus, in Chapter VIII the argument of the first two Postulates of Empirical Thought is effectively turned against any attempt to assert their real possibility or existence. But then, of course, Kant himself was never guilty of such an attempt: it was, after all, *his* thesis, as well as Cassirer's, that the notions of possibility and actuality have a phenomenal meaning only! Kant is again misconstrued (in my opinion), and the argument of the Analogies discounted, when it is argued in Chapter IX that he had recourse to the notion of the thing-in-itself chiefly in order to guarantee the reality and objectivity of sensible existents. Likewise, throughout this Chapter, Kant's explicit statements about the problematic or negative character of the 'purely limiting concept of noumena' are made to count for

nothing. What is perhaps most interesting in all this is the appearance of novelty that is given to Cassirer's contention that Kant's theory of things-in-themselves can be rendered acceptable only by connecting it as closely as possible with the notion of an intuitive understanding, and his suggestion that the term 'transcendental object' in fact signifies "nothing more than sensible reality as it would reveal itself to a mind capable of intellectual intuition" (p. 289). The point is, I think, a sound and enlightening one, but it is an exposition of, rather than an amendment to, what Kant says; compare, for example: "if I postulate things which are mere objects of understanding, and which, nevertheless, can be given as such to an intuition, although not to one that is sensible—given therefore *coram intuitu intellectuali*—such things would be entitled *noumena (intelligibilia)*" (A 249). Although Cassirer makes great play with the notion of intuitive understanding, I fail to see that he makes it any more illuminating than Kant has already done.

Cassirer's treatment of the Transcendental Dialectic, in the remaining chapters of the book, is pretty straightforward. While disputing Kant's diagnosis of the *origin* of the transcendental illusion, and rejecting the logical framework within which Kant works, he for the most part concurs in principle with Kant's therapeutic recommendations. Thus, in the next three chapters, he traces with Kant the transcendent use of Reason in the spheres of Rational Psychology, Cosmology and Theology, and concludes, in Chapter XIV, with an account and estimation of its immanent or purely regulative use in the systematisation of our knowledge (as treated in the Appendix to the Dialectic). It is in this section of his work that Cassirer's method approximates most closely to that of more orthodox commentaries, and a lot of the points raised in his discussion are very enlightening indeed. Clearly, much of the 'permanent significance' of the *Critique* consists for the author in the effectiveness of its criticism of metaphysics.

An overall evaluation of a book as complex and ambitious as this would, I feel, be inappropriate. Although its general concern is with the *reconstruction* of Kant, its worth consists largely in its instructiveness on particular points in Kantian doctrine. And while I suspect that salvage operations of this kind will be unlikely to recommend themselves in entirety to contemporary vendors of philosophical truth, they do at least serve to redirect our attention to lessons of the past which many of us have forgotten and some have never learnt.

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MORAL JUDGEMENT. By D. Daiches Raphael. London, Allen & Unwin, 1955. 224 p. 16s. (U.K.).

In his recent book *The Logic of Moral Discourse*, Paul Edwards refers frequently to "Ross and Raphael" as a pair of arch-intuitionists, and so the resolute opponents of naturalism in ethics. But it appears from Dr. Raphael's latest book that, although still much influenced by Ross and Carritt, he is feeling his way towards a cautious naturalism, a little hesitantly, and certainly with much more appreciation than Edwards of the difficulties in the way.

This book is divided into two sections. In the first, "Logic of Morals", Raphael sets out to examine the implications of ordinary moral judgements. These are treated as the data which are to be "explained" or fitted into a coherent system. The principles of this system are to be tested by the two criteria of coherence and accordance with the data. Raphael concludes that, of all the schools of moral philosophy, the deontologists have had most success in constructing a system which satisfies these tests. "Right" cannot, he thinks, be defined in terms of "good"; other obligations cannot be reduced to the single obligation of promoting happiness (or, more generally, good); moreover, this last is not strictly an obligation at all, since it has not the same stringency as the obligations to pay debts, keep promises, look after one's children, etc., etc. There are, then, two irreducible concepts involved in common sense morality: the obligatory and the fitting. It is fitting (but not obligatory) to increase happiness, and to "produce other good states of mind, namely virtue, knowledge and the creation and appreciation of beauty"; it is obligatory (and also fitting) to relieve pain, tell the truth, show affection for one's friends and promote justice. Justice is analysed in some detail: it includes keeping contracts, rewarding merit, ensuring equality of liberty and of opportunity. Since right actions are always at least fitting (and sometimes obligatory as well) and the good is what it is fitting to pursue, we do not need these terms as well as the two basic ones.

This is, except in some of its details, a familiar position; and, on the whole, Raphael defends it with familiar arguments. A utilitarian might indeed object that he does not always consider the stock utilitarian replies to these arguments: for example, one of the central issues between utilitarians and deontologists, the alleged impossibility of reducing contractual obligations to the duty of increasing the general happiness, is discussed only very briefly. More space is given to another central issue, the difficulty of justifying punishment solely by reference to social

utility; but here Raphael concedes more to utilitarianism than he seems to realise. For he agrees with Sidgwick that there are two utilitarian principles and not merely one: the equal distribution of happiness as well as its maximisation. He recognises, too, that the conflict between justice and expediency ("It may be *expedient*, but not just, that one man should die for the people"—p. 73) is a conflict between these two utilitarian principles. The problem arises, then, within utilitarianism itself, and does not prove that other basic principles are needed besides these two. If we interpret "equal distribution of happiness" to mean "equal opportunity of happiness" (which would be in line with what Raphael says about "equality of liberty") we can argue that our reason for punishing only the guilty is to ensure that everyone will have an equal opportunity of not being the man who has to die (or at least suffer) for the sake of "the people". There is no need, then, to invoke an independent obligation to distribute happiness strictly according to moral worth. It seemed self-evident to Ross that we have such an obligation; but to many of us the whole notion is morally repugnant. Only a prig would worry about whether a happy man deserved his happiness, unless he felt that others were being made miserable as a consequence.

On the whole, this first part of the book is a careful re-statement of the deontological position, rather than a defence of it against utilitarianism: as such it has considerable value. But Raphael realises that a mere systematisation of the moral judgements of common sense leaves many questions unanswered. If we treat common-sense moral judgements in the way the scientist treats physical phenomena, as data to be ordered into a coherent system, we are arbitrarily overruling the claim of the moral reformer, that some of these common-sense judgements should be rejected altogether. Raphael tells us that "the logic of morals" does indeed modify common-sense morality, by making it more precise and by removing any internal incoherence; but farther than that it cannot go. It is left for the metaphysic of morals "to find a *rationale* for the system as a whole, to compare it with other systems and to ask how far it is consistent with the presuppositions of systematic thought in other fields (e.g. of natural science)". It is here that naturalism becomes relevant.

In his introductory chapter Raphael tells us that in the first part of the book he "sides with deontology as against utilitarianism" and that in the second part he rejects intuitionism, at least in its common form, arriving at a theory which might be called "deontology without intuitionism" or "Kant in naturalistic dress". It seems to me, however, that what he actually does in

the second part is to side *with* utilitarianism, and that his adoption of naturalism (which he says is tentative) does not go nearly as far as he seems to think.

Moral obligation, Raphael tells us, arises from the human capacity for sympathetic insight into the feelings of others. Animals have no obligations, because they do not have this insight; but we have obligations to animals, because we have some limited appreciation of their feelings. This sympathy creates a "bond" or "interpersonal relation"; the obligation to which it gives rise is ultimately "to treat others as ends"; and this means, primarily, to regard their desires as of equal importance with our own.

What has become here of the distinction between the "obligatory" and the "fitting"? Raphael's rejection of utilitarianism depended on this, together with his assertion that it is merely fitting, and not obligatory, to promote the general happiness. It now appears that the distinction is not, after all, an ultimate one: it belongs merely to "the logic of morals", to the moral consciousness of the average European. "Beyond the level of the obligatory is the sphere of fitting or good action that is not (at any rate yet) generally regarded as obligatory." (p. 116). "Our capacity to serve the interests of others is limited Therefore implicit conventions have arisen that only in certain types of situation is altruistic action strictly obligatory as opposed to fitting, and that in such-and-such a set of circumstances my obligation to A is greater than my obligation to B. These conventions are usually due to the relative strength of people's natural dispositions or to proved social utility, and often to a combination of both." (p. 125).

Now this is what the utilitarian has always said: that the "irreducible obligations" of the deontologist are really rules of thumb about how to promote happiness, allowing for "natural dispositions" and the particular structure of the society in which we find ourselves. No utilitarian would object to the distinction between the fitting and the obligatory so long as this is regarded as merely part of "the logic of morals", i.e. as a purely conventional one whose ultimate *rationale* is the greatest happiness of the greatest number. Raphael, it is true, does not talk about the greatest happiness of the greatest number; but since he takes "interest" or the satisfaction of desire as the end of moral action, since he says that all men's desires have an equal claim to be considered, and since he denies that any desires are bad as such (they are bad only in so far as they "involve the thwarting or

stifling of others . . . in the individual himself or in other persons"—p. 122) it is hard to see much difference between the "humanist ethic" he advocates and utilitarianism.

It seems to me, then, that Raphael's claim to be defending the deontologists against the utilitarians is highly misleading: when he comes to "the metaphysic of ethics" (which is, after all, what utilitarians have usually been talking about) he seems to change sides. What about his other claim, that he finds a naturalist basis for his deontological views?

Raphael offers us a naturalistic account of "good": "There is, it seems to me, a grain of truth in Hobbes's view that what is called good . . . is the object of desire. But Hobbes was mistaken in making the definition egoistic by restricting a man's use of the word 'good' to what satisfies his *own* desire." (p. 118).

It seems to me that this misses the real point of Hobbes's naturalism. Human desire is, for Hobbes, a biological phenomenon: it can be compared to the flight of the moth towards the light, or the turning of the plant towards the sun. The difference is that, in human beings, this purely physical phenomenon is also reflected in consciousness. We are not, of course, aware of it *as* a physical phenomenon: we are not directly conscious, for example, of contractions of the stomach, but of the sensation we call hunger. Similarly, we are not aware of the biological causes of desire, but only of a particular attitude towards the desired object: it is this that we express by calling it "good". In the same way we might imagine a moth, completely ignorant of "phototropic cells" or whatever it is that biologists talk about, saying that light is good. Obviously this is a purely naturalistic account of "good".

But this explanation loses its force if we say, as Raphael does, that "good" refers to the object of *anyone's* desire, to the desires of others as well as ourselves. Of course, we can say, as Hobbes does, that what another desires will be good to him, or his good; but we cannot say, without begging the whole question, that the object of another's desire will be good to me. This is to go beyond naturalism; for it is to say, not simply that I do desire that others shall attain their ends (for this is often false), but that I ought to desire that others shall attain their ends. And if this is what is meant, we are no longer using "good" in a naturalistic sense.

We cannot overcome this objection by saying that it just is a fact that human beings have sympathetic insight into the strivings of others. For what follows from this? According to Raphael, the "bond" of obligation arises out of this fact; but

what does this mean? Not that we always do treat others' ends as our own, whenever we have this insight; for often we don't. A deontologist might say simply that we ought to; but this is, once again, to abandon naturalism. Raphael seems inclined to say that we feel that we ought to; and that this is simply a fact, a psychological phenomenon. I say that he is *inclined* to say this; but the inclination does not carry him very far. "To think that we *ought* to pursue his ends is to strive to feel towards his ends as we should naturally feel towards our own. The conation thus evoked is what is referred to by the idea of moral obligation. To say that I am obliged is not to say that I *have* this conation. Still more obviously, to say to or of another man that he is obliged is not to say that he *has* the conation. Once the *idea* of a (metaphorical) bond to another person has arisen, it has its own meaning of, simply, a bond or requirement." (p. 125; his italics). "Requirement" here would seem to mean that we *ought* to "have the conation" whether we do have it or not; and it is hard to see how a naturalist could say this.

A very similar difficulty arises in Raphael's discussion of the Kantian principle about treating others as ends. As we have seen, he interprets this in the utilitarian way, to mean that we should strive to realise the desires of others. But of course this is certainly not all (and perhaps not at all) what Kant means by it. The other interpretation is that we should strive to realise, not the desires, but the moral aspirations of others. Raphael tries to combine these two interpretations: "To think of others as persons, as entering into moral relationship with us, is to think of two different things; first, it is to think of them as having interests and of the possibility of our helping to satisfy those interests; secondly, it is to think of them as moral agents whose judgements we should respect as laying down the moral law not only for themselves but for everyone We have no prerogative of moral judgement; in that respect, too, all men are equal." (p. 134).

Now if men's moral judgements are just feelings that they happen to have, like their "natural desires", then it seems reasonable to extend the utilitarian interpretation of Kant's maxim and to say that we should take account, not only of all men's desires, but also of all men's moral judgements. But the obvious difficulty here is that Kant's maxim itself must, on this view, be treated as just one moral judgement among others. So that the over-riding principle is not, after all: "Treat others' ends as equal in value with your own", but "Arrive at some compromise between treating others' ends as equal with your own and such other

moral principles as treating others as means to your own ends, treating only the ends of superior persons as of any value, etc., etc." But if we accept the compromise there is no longer any reason to accept the compromise; since we accept it only so far as we regard the original maxim as the only valid moral principle. Clearly this is an untenable position. On the other hand, if moral judgements are not just feelings that people happen to have, but propositions which may be true or false, it would seem to follow that we take account of them only so far as they are true, and that men are not, after all, "equal in this respect". Of course it may be said that the truth is difficult to discover, and that a wise man will take account of others' opinions; but this is not to respect the moral judgements of others, as we respect their desires, just because they are theirs; it is to respect them because they may be true.

In a later chapter Raphael tries a different approach: he considers the view that fundamental moral judgements are not attitudes which people happen to have, but "postulates", like the axioms of a mathematical system. He points out that rationalists have always regarded moral judgements as somehow analogous to mathematical ones and that there seems no reason why empiricists should dispute this claim, since they themselves do not deny the validity of the propositions of mathematics. They do disagree with the rationalists about the nature of these propositions; but, Raphael argues, if we accept the empiricist account of mathematics and apply it to morals, we need not deny that we *know* moral propositions; we do not have to say that they are really "feelings" or "attitudes" masquerading as knowledge. On this view an ultimate moral judgement would be, not a self-evident intuition, but "a postulate, a general policy of action, to which there could be alternatives". But this phrase does, I think, reveal a difficulty. Can a moral principle be merely a postulate and at the same time a policy for action? We do not merely entertain an ultimate moral principle in order to see what would follow from it; in some sense we commit ourselves to it, we assert or accept it. And this is not like acting on an assumption in order to see if it works; there is here no independent criterion to tell us what works and what does not: the function of a moral principle, indeed, is to provide such a criterion. No doubt mathematical postulates, too, cannot be verified; but a mathematical system as a whole can be said to "work" if it enables us to explain phenomena which alternative systems cannot explain: the test is not merely internal coherence.

Raphael does indeed try to find a similar test for alternative moral systems. "We might take each policy by itself, accepting its own premisses for the sake of argument, and see if it can be realized in practice. There are two ways in which it might fail to do so. First, we might find that human beings could not in fact use the policy as a guide to action. Secondly, a policy that could be pursued might lead to results inconsistent with its own aim." (p. 186). But his attempts to apply these tests are not very convincing. For example, he tells us that egoism passes the first test but fails the second. "There is a familiar objection to egosim, which I think is supported by reasonably good evidence, that to aim at happiness is to miss it. A man who simply keeps his eye on the pursuit of his own happiness is apt to find, at the end of his life, that he has not attained his ideal and might have come nearer to it if he had not pursued it directly." (p. 187). I am not sure whether Raphael is referring here to "the paradox of hedonism" (that the way to get happiness is to think about something else) or is making the Hobbist point that our interests, at least in a society, are so bound up with those of others that the far-seeing egoist will find it wise to think of others as well as himself. It is quite clear that neither of these disposes of egoism as a policy: they merely go to show that the actions which follow from adopting the egoist policy are not what the hasty observer might expect. The question between the egoist and his opponents is whether we ought to consider the interests of others for their own sake or for the sake of our own happiness; and it is hard to see how this question could be settled by the kind of argument Raphael is using here.

In the end Raphael tells us, with exemplary honesty, that "I cannot make up my mind whether to adopt, in the final analysis, the position of the intuitionist or that of the naturalist." (p. 190). And indeed it seems to me that throughout the book he is taking uncertain steps towards naturalism and then backing away again. He is not to be blamed for this; the objections to either position are formidable, and those who advance confident solutions generally convince themselves rather than their readers. If he does not reach any final solution, Raphael does give us a careful discussion of many important problems, and usually manages to be illuminating. One might mention, as instances, his critique of Moore on the naturalistic fallacy on pp. 37-43, his analysis of justice on pp. 62-94, his whole discussion of the distinction between the "logic" and the "metaphysic" of morals, and his argument, on p. 122, against the contention that "Why ought I to do my duty?" is a pseudo-question.

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THEORY OF GAMES AS A TOOL FOR THE MORAL PHILOSOPHER.

By R. B. Braithwaite. Cambridge University Press, 1955.
76 p. 6s. (U.K.).

This short book is Braithwaite's inaugural lecture as Knightbridge Professor of Moral Philosophy at Cambridge. In it he considers a rather stylised situation in order to show how the theory of games may be applied to moral philosophy, or, more accurately, to casuistry. He considers two persons, Luke and Matthew, whose interests are partly opposed. They are bachelors and occupy adjacent flats, without sound-proofing. They each have only one hour, from 9 p.m. to 10 p.m., for recreation, and Luke's amusement is to play classical music on the piano, while Matthew's is to improvise jazz on the trumpet. Luke has certain preferences. His first preference is for himself alone to play, his second preference is for Matthew alone to play, his third preference is for neither to play, and the thing he likes least is for Matthew to trumpet while he himself is playing the piano. The interval between his first preference and his second, the interval between his second preference and his third, and the interval between his third preference and his fourth are in the ratio of the numbers 1, 2 and 3. (Braithwaite shows how in suitable circumstances a meaning can be given to this numerical statement, following a method elaborated by Von Neumann and Morgenstern.) Matthew's preferences are firstly for himself to play the trumpet while Luke is silent, secondly for Luke alone to play, thirdly for both of them to play (since he likes a cheerful noise about the house), and lastly for neither to play. The intervals between preferences in his case are in the ratio 1 to 1 to 7. This situation is a competitive but not wholly competitive one, and so the ordinary theory of zero-sum two person games is not applicable. There is a 'cooperators' surplus' to be shared, and Braithwaite shows how a decision on this point can sensibly be made. Von Neumann has shown that each player has a certain prudential strategy (of playing his instrument on a certain proportion of evenings a week, choosing by a suitable random device, which preserves the given proportion, just which evenings to play his instrument) which will secure himself the maximum benefit compatible with any choice of strategy by his opponent. However, this prudential strategy merely provides a lower bound to the most acceptable strategy: Von Neumann's methods do not show how the cooperators' surplus can best be shared. It is for situations of this general sort that Raiffa has proposed a method of solution, and Braithwaite's method largely

follows his. However, Raiffa did not see any way of dealing with a situation (like the present one) in which the relative preference scales of the two persons are not expressible in terms of a common unit. What is new in Braithwaite's method is that his analysis gives rise to a natural utility unit which is plausibly suggested by the logic of the collaboration situation. It is impossible in a short space even to give an outline of the argument, but Braithwaite's calculations lead up first of all to a provisional recommendation that Luke and Matthew should 'come to an agreement that they should each play their instruments in specified proportions, so that Luke can rely upon Matthew's not trumpeting more than $59/104$ ths of the time if Luke confines his playing to $23/68$ ths of the time, and vice versa.' (p. 32). These are not the prudential strategies in the sense of those that will secure the maximum advantage to each compatible with complete ignorance as to how the other will act, but they are strategies which require the minimum of mutual trust in that if either deviates from the recommended strategy the other can fall back towards his independent prudential strategy, and will never lose more than he would lose if he had used his independent prudential strategy in the first place. Strategies depending on this minimum of mutual agreement Braithwaite calls 'paired strategies'. If Luke and Matthew accept his argument so far and agree to this provisional recommendation, then Braithwaite can make a final recommendation that Luke and Matthew should adopt a 'coupled strategy', according to which Luke and Matthew agree that Luke should play the piano on 17 evenings out of 43 and that Matthew should trumpet on the remaining 26. In this way they each get more enjoyment than they could with the paired strategies, and with the paired strategies, again, they get more enjoyment than if they stuck to their ordinary Von Neumann prudential strategies.

The two men get the most advantage by using a coupled strategy, and it is argued that neither will object to adopting the coupled strategy, as it gives each man more advantage than he could get by using non-coupled strategies. Nevertheless the introduction of coupled strategies may lead to conceptual difficulties reminiscent of Hobbes' *Leviathan*. This can be seen if we consider a simple game devised by my colleague Dr. B. C. Rennie to illustrate this point. Consider a two-person game which consists of exactly 100 moves for each player, the moves to be made alternately. Each player at each move has two alternative choices, the unselfish choice *U*, which gives himself and the other 2 points each, and the selfish choice *S*, which gives himself

3 points and the other nil. Common sense suggests that one player should go on playing *U* as long as the other does. In this case they can ideally each gain 400 points. However, consider this argument: the player with the last move will certainly play *S* irrespective of what has gone before, and his opponent, knowing this, will play *S* the move before this, and so on for the whole game, so they each get 300 points instead of the 400 they might have got.

Even the theory of, say, the zero-sum two person game can give rise to conceptual problems. Von Neumann suggests that in this case rationality is to be equated with maximising one's minimum profit. We could criticise this definition on the grounds that it does not take account of the variance of the possible profits. The relation between the definition of rational behaviour in the theory of games and our ordinary intuitive conceptions of rationality is thus a field worthy of investigation. I suggest that, whether or not the theory of games is a useful tool for the moral philosopher, philosophical analysis is an indispensable tool for the theorist of games (or at any rate for the theorist who wishes to apply his theory to making practical recommendations).

I doubt whether the theory of games, as it stands, can be a tool for the moral philosopher. As we have seen, it deals with competitive or partly competitive situations, and even when coupled strategies are allowed these are only brought in to help each participant the better to maximise his own profit. The theory is essentially egoistic. In morality, on the other hand, we are concerned with maximising the *general* good. (I speak as a utilitarian.) However, the moral situation is not always that of a *simple* maximisation problem. In order to determine what action will maximise the general good we have often to take into account what the behaviour of other people will be. We do not know how to behave unless we know how they will behave, and they do not know how to behave unless they know how we will behave. There is a circularity involved which can be got over only with the type of approach found in the theory of games. Thus consider four motorists converging on a cross-roads in a country without traffic rules. The maximum total benefit is got by one person going on and the other three stopping. However, the most rational behaviour is for all to stop, as the solution to the problems of behaviour must in this case be a symmetrical one. (I am here neglecting the possibility of a mixed strategy, that is of each deciding to adopt a very small probability of going on.) A motorist who goes on cannot assume that another may not go on too. Though one going on and the other

three stopping is the best possible combination of choices, if two go on we get a crash, which is a much worse result than the delay of all four stopping. Nevertheless I suspect that the solution of moral problems of this sort will always be trivial: I doubt if the theory of morality (of maximising the general good) would develop the complexities of the theory of egoistic behaviour (the theory of games).

Whatever the philosophical relevance of the theory of games may be, this book is quite clearly a definite contribution to mathematics. Braithwaite is especially ingenious at constructing geometrical models for the theory of games.

J. J. C. SMART.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

AYER, A. J., and ors. *The Revolution in Philosophy*. London, Macmillan, 1956. 126 p. 17s. 6d. (Australian).

A series of lectures, originally given over the B.B.C.'s Third Programme, on contemporary developments in philosophy, beginning with Bradley and continuing through Frege, Russell, Wittgenstein in his early period, and the Vienna Circle to the later Wittgenstein and his followers. Authors are: Ryle, Wollheim, Kneale, Pears, Paul, Ayer, Strawson and Warnock.

BARBU, ZEVEDEI. *Democracy and dictatorship; their psychology and patterns of life*. (International Library of Sociology and Social Reconstruction.) London, Routledge and Kegan Paul, 1956. viii, 275 p. 28s. (U.K.).

DOTTERER, RAY H. *Postulates and implications*. New York, Philosophical Library, 1955. vi, 509 p. \$7.50.

Discusses the place of axioms and postulates in rational thinking, goes on to lay down the postulates which the author thinks justified ("that I am not the only self which exists", "that maximum fulfilment of desire should be treated as the greatest intrinsic good", etc., etc.) and so proceeds to give his views on epistemology, religion and ethics.

KANT, I. *Critique of practical reason*, translated with an introduction by Lewis White Beck. New York, Liberal Arts Press, 1956. xxxiv, 168 p. 90c.

KIMPEL, BEN. *Moral principles in the Bible; a study of the contribution of the Bible to a moral philosophy*. New York, Philosophical Library, 1956. 172 p. \$4.50.

KRONER, RICHARD. *Kant's Weltanschauung*, tr. J. E. Smith. Chicago, University of Chicago Press, 1956. xi, 119 p. \$2.

"An authorized translation with revisions by the author, [this] is one of the first examples available in English of the so-called Heidelberg interpretation of Kant. The Heidelberg school puts greatest stress on the moral and religious aspects of Kant's thought . . ."—Dust-jacket.

LIU WU-CHI. *Confucius, his life and time*. New York, Philosophical Library, 1955. xv, 189 p. \$3.75.

LODGE, RUPERT C. *The philosophy of Plato*. London, Routledge and Kegan Paul, 1956. ix, 347 p. 28s. (U.K.).

"I try to help the reader to open his mind to the inner essence or 'Platonic idea' of all parties to Plato's discussions. Thus, as parties to most discussions in the Dialogues, we find something like the following: (1) Plato's idea of Hellenic commonsense views; (2) Plato's idea of Hellenic factual science (Heracleitus); (3) Plato's idea of mathematical science (the Pythagoreans); (4) Plato's idea of the humanist movement (Protagoras, Georgias, and others); (5) Plato's idea of critical idealism (Socrates) In this way I try to bring out what Plato *as Plato*, in addition to depicting the views of the others, is doing."—Preface.

MARCUSE, HERBERT. *Eros and Civilization; a philosophical inquiry into Freud*. London, Routledge and Kegan Paul, 1956. xii, 276 p. 25s. (U.K.).

Criticises, on the basis of Freud's own assumptions, the thesis of *Civilization and its Discontents*, that civilization requires the rigid restriction of "the pleasure principle"; and, in so doing, attacks the "revisionist neo-Freudians" (Fromm, Horney, Sullivan) who, the author argues, have made psycho-analysis respectable by ignoring the criticism of society implicit in it.

MOSSONSEN, DAVID. *A history of teacher training in Western Australia*. (A.C.E. Research Series, no. 68.) Melbourne, Australian Council for Educational Research, 1955. x, 116 p. 19s. (Australian).

NEURATH, O., and ois. eds. *International Encyclopedia of Unified Science*, volume 1. Chicago University Press, 1955. ix, 760 p. \$11.

Cloth-bound edition of a well-known series of monographs by Neurath, Carnap, Morris, Russell, Nagel and others. This first "volume" is actually in two volumes.

PIKE, KENNETH L. *Language in relation to a unified theory of the structure of human behaviour*. Part 2, Preliminary edition. Glendale, California, Summer Institute of Linguistics, 1955. v, 85 p. \$2.

RUNES, DAGOBERT D. *On the nature of man; an essay in primitive philosophy*. New York, Philosophical Library, 1956. 105 p. \$3.

SCHOENFELD, CHARLES. *God and country*. New York, Philosophical Library, 1955. vi, 119 p. \$3.

An analysis, from the point of view of psycho-analysis, of current American institutions and practices, especially religion and patriotism.

SYDNEY UNIVERSITY. *Department of Tutorial Classes. Some papers in adult education*. Sydney, 1956. 112 p. 4s. (Australian).

URMSON, J. O. *Philosophical analysis: its development between the wars*. Oxford, Clarendon Press, 1956. x, 203 p. 33s. 6d. (Australian).

Traces the development of the analytic movement from the Logical Atomism of Russell and the early Wittgenstein, through Logical Positivism to the current linguisticism.

WOLFARD, MERL RUSKIN. *Thinking about thinking*. New York, Philosophical Library, 1955. x, 273 p. \$5.

The author, who is an engineer, tries to develop "a scientific theology" or "mentality engineering procedures" as a means of dispelling "the psychic confusion" of "a skepticized society" in which, he says, "critical thinking is rampant". Discursive and journalistic in style.

THE AUSTRALASIAN JOURNAL OF PHILOSOPHY

VOL. 34

DECEMBER, 1956

No. 3

RELIGIOUS DISCOURSE AND THEOLOGICAL DISCOURSE¹

By R. F. HOLLAND

What are we talking about when we talk about God? I suppose that, as a formulation for the main problem in the philosophy of religion, this to many philosophers would seem by no means odd. Some might even take the posing of the problem in this way to be a sign of respectability. Yet, if I enquire what it is we are talking about when we talk about God and my aim is to discover what it is for people to be religious and to believe in God, then it is probable that I have fallen already into confusion. For what is it *who* are talking about? Who talks *about* God?

It might be answered, "Religious people of course — those who believe in God". But now: *is* it in fact usual for seriously religious persons to do anything that can be properly described as talking about God? They believe in God certainly; and they are usually ready to affirm this belief. They worship God, thank God, praise God, ask God's forgiveness. Perhaps they could be said to talk *to* God: but that is not the same thing as talking *about* God. If in addition to talking to God they may also sometimes talk about God, it is not obviously essential to their way of life, to their religious belief and to the carrying on of their religious practices that they should do the latter. God is not necessarily for them a topic of conversation, a subject for discussion and enquiry, either generally or when their discourse is at its most elevated level.

Religious believers, as such, ought not then to be included without question among those who commonly talk about God, although a motley collection of people unquestionably can be included — popular scientific figures and Third Programme humanists, for example; also psychoanalytic theorists, philosophers, some mystics possibly; but, above all, professional

¹ This paper was read to the Northern Universities Philosophical Society at Shrewsbury in April, 1956. It owes much to discussions with Mr. R. Rhees, and in particular the idea of stressing gratitude for one's existence as the primary ingredient in a religious attitude.

theologians. I have in view here that aspect of theology which the derivation of the word implies — the provision of a λόγος of God, i.e. the attempt to give an account of God, somewhat as an account may be given of birds in ornithology and of insects in entomology; the eliciting and arrangement of the facts about God. Whether the source of the facts be supposed to lie in reason itself, in the empirical nature of the universe and inferences therefrom, in revelation or in any combination of these things, is immaterial. What makes it theology is that God should be enquired into and more or less systematically studied; that a kind of description or analysis or theory should be attempted which, though it may be claimed to be of or about an exceedingly special something, is nevertheless assumed to be *about* something in a quasi-scientific sense (theologians have traditionally called theology a science).

I think it is a natural extension of the use of the term 'theology' to make it cover not only the discourse of self-styled theologians, whenever they talk or argue about God, but also the — from one point of view anti-theological — discourse of atheists, agnostics, sceptics, humanists, etc., whenever *they* talk or argue about God. These two types of discourse, *theistic theological discourse* and *antitheistic theological discourse* as they might conveniently be labelled, belong to the same logical category. The confounding of antitheistic theological discourse with religious discourse would be an error which nobody is inclined to commit. But theistic theological discourse is quite commonly treated as if it belonged to the category of religious discourse and even sometimes as if it were coextensive with it. This mistake sets arguments about the relation or lack of relation between religion and science completely askew, and it is the greatest impediment to progress in the philosophy of religion.

I have so far characterized religious discourse very roughly by reference to the prayer and worship of 'the seriously religious person' or 'the believer as such' — the language in which God is spoken *to* rather than *about*. But this characterization is already open to misconstruction in at least two ways. In the first place, I may be taken to be setting a premium upon the man-in-the-street, or perhaps it should be in this case the man-in-the-church or the man-on-his-knee, a being supposedly venerable for his uncorruption by theology in the way the man-in-the-street is sometimes sanctified because of his alleged freedom from metaphysics. Let me say then that I hold no brief for the plain man or plain believer and that what I invoked was an idealized version of him as a temporary (persuasive) expedient; for as a matter of fact I should expect to find the majority of his kind

in a state of confused and unconscious oscillation between the two types of discourse. However, the analogy between the two kinds of corruption seems to me to hold, and in the sense in which it is proper to regard metaphysics as being, in general, a product of confusion it seems to me also proper to regard theology² in general, theistic and antitheistic alike, as a product of confusion. In the second place it is not, as I might have seemed to suggest, a necessary feature of religious discourse that "God" should appear in the vocative or only in second-person statements. Religious people reflect and meditate as well as pray. And out of the variety of statements they may make — which may concern for instance the interpretation of historical happenings, or form part of certain traditional stories and parables, or belong to the reflections and judgements made by a religious individual upon his personal life and conduct — there are some which might seem to be egregiously about God. These are the statements which function, so to speak, as the high-level generalizations of religious expression, summarizing the believer's attitude to his existence and focusing it directly into an attitude of worship. For example, it must be possible for a religious person to believe and to assert that God is his Creator, to Whom everything is owed; or that God is the God of mercy, of Whose forgiveness he stands in need. But if such statements are statements about God then they are so only to something like the extent to which an assertion that I owe my life to my parents who begot me would be an assertion about my parents, or an assertion that I stand in need of some condemned person's forgiveness would be an assertion about that person. I should prefer it to be said that they are statements not so much about God as about the believer: or perhaps that they are statements about the believer's relationship to God. But if someone were then to request a description of that to which this relationship is a relationship, I should say that this request signified a misunderstanding.

Consider the following two passages from *The Devil* (*Notes for a future Diabology*), by Giovanni Papini:

- A. "God has defined himself: I am He Who Is. Were the Devil his absolute opposite, he should be identical to Nothing. But were he Nothing . . . dialectical play

²The analogy with metaphysics seems to me to be relevant when theology is said to be, in the special sense of this term that Wittgenstein introduced, *grammar*. Metaphysician = grammarian with illusions about what he is doing, hence a misrepresenter of grammar. I speak here of theology and metaphysics *in general*. That genuine illumination can be derived (by one who is careful not to be a metaphysician or a theologian) from particular metaphysical and theological assertions I would not deny.

would force one to argue that the Creator, having created the world from Nothing, had drawn it indeed from the Devil and that the very stuff of the material universe is diabolic." (p. 58-9. The translation is by Adrienne Foulke.)

- B. "We do not think enough about this infinite grief of God. We have no compassion for God's torment. Most of those who do recognize that they are his children, make no effort to understand or assuage God's boundless affliction. We ask gifts, intervention, or forgiveness of the Father, but no one shares, with tender, conscious filial affection, in God's perpetual anguish." (p. 42.)

The first of these passages is obviously theological and highly so. I would say that the second by contrast bears many of the marks of religious discourse, though it may (it is hard to be completely sure of this) be theological also in a different fashion.

In *A*, God is presented as a *quasi-scientific* creator—the maker of a *material* universe (as opposed to some other sort of universe ?), and moreover the maker of it out of some 'stuff', rather in the way that bricks might be made out of straw. In addition to its strong quasi-scientific element, this same passage exemplifies another common feature of theological discourse, which might be designated for convenience its *quasi-deductive* element. Papini supposes that He Who Is might, because certain other things do, have an opposite which, like the opposite of anything else that is, would be something that isn't, only in this case very emphatically something that isn't—in other words *Nothing*, with a capital *N*. Theology has traditionally proceeded in a manner vaguely suggestive of the proof or refutation of geometrical theorems and with much juggling of such notions as *being* and *not-being*, *existence* and *essence*, *substance* and *attribute*, *infinity* and *finitude*, *necessity* and *contingency*, *cause* and *effect*. The present example is admittedly of a low-grade kind; higher-grade theology is less obviously akin to Lewis Carroll.

Passage *B* is neither quasi-scientific nor quasi-deductive. But Papini's book as a whole reads as if it had been designed as a piece of entertainment rather than as a religious meditation (and though this intention is denied in the preface it is denied with journalistic immodesty). One is therefore uncertain whether his complaint, in this particular passage, of our lack of compassion for God's torment and of our failure to share in God's perpetual anguish is a piece of artistry, a theatrical display and perhaps a self-gratifying discharge of emotion; or whether it might spring

from a deep sense of compassion, yet sinful lack of compassion, and from an active striving to share God's anguish, not theoretically or imaginatively, but in life. Passage *B* then may or may not exemplify a third and most pervasive characteristic of theological discourse, which I shall call the *quasi-aesthetic*.

The quasi-aesthetic character of theological discourse appears in various guises of widely differing degree of subtlety. But it may be said to have two essential prototypes:

1. 'The greedy nephew's conception of God', *i.e.* the representation of Our Heavenly Father as if he were our rich uncle, who by waving a wand will bestow on us now or later—possibly after we are dead—a wealth of benefits.

2. 'The connoisseur's conception of God', *i.e.* the kind of contemplation or admiration of God which is carried on as if the participants were Professors of the Fine Arts considering the properties of some exquisite bric-à-brac.

The worldliness, the obvious degradation of the former and the (by contrast) somewhat refined and in one sense unworldly or spiritual tone of the latter can cause the gulf between these two conceptions to look much greater than it is. The second is no less pagan than the first and its particular brand of elevation has nothing to do with religion. The connecting link between the two conceptions consists in the fact that each would turn God into a possible object or source of enjoyment: in each case, though in a different way, there is an attempt to utilize God. The antitheistic theologian's version of quasi-aesthetic theology consists often in repudiating the statements of the theistic theologian under the erroneous impression that these statements are religious statements and then finding a source of enjoyment in God's non-existence. He may go on to claim that the 'kick' that is to be got out of atheism or irreligion is a more adult kind of 'kick' than the 'kick' that is to be got out of religion. But I do not see how the atheist who gets a 'kick' out of atheism can any more understand what atheism is than the man who gets a 'kick' out of theism understands what theism is.

Quasi-aesthetic theology is not constituted simply by the use of aesthetic language in connection with religion. The psalms and Christian hymns are stuffed with aesthetic language, with poetic imagery, but here the aesthetic preserves its own identity at least to some extent intact, while being pressed more or less openly into the service of religion—as is more obviously the case with religious music and religious painting and architecture. Such devices, whether powerful or feeble, I would rather call *aesthetic* than *quasi-aesthetic*: their influence upon religion can

indeed be damaging, but it takes an inordinate degree of stupidity to mistake them for religion itself. The typically quasi-aesthetic, *i.e.* the aesthetic that masquerades as something else, is to be found in certain kinds of solemnly propounded, intellectual-seeming talk, where the aim is to impart religious instruction and the intention of producing a state of merely emotional receptivity in the hearer would be disclaimed. It is here that the destruction of religion is mainly wrought.

A notable exponent of quasi-aesthetic theology is Professor C. S. Lewis, and I shall refer for an example to the series of talks which he published under the title *Beyond Personality*. The central theme of these talks is 'God the Divine Artist'. This is a popular variant of the connoisseur's conception of God, and Professor Lewis combines it very smoothly with the greedy nephew's conception in an argument which appears to be of the following kind: God is the maker of all things bright and beautiful. We as God's artifacts are potentially bright and beautiful, though not yet actually so, since our state at present is that of unfinished work. But we are also artists ourselves and hence can have communion with God in art. In other words, the Godly part of us is identical with the aesthetic part, our spiritual vicissitudes aesthetic vicissitudes, our possibility of transition from worldly existence to Eternal Life a possibility of transition from one aesthetic state to another (from the bad dream to the excitement of the morning, from feebleness and filth to energy and radiance, from boredom to fun). The more we cultivate our aesthetic sense, the more we can appreciate what the Divine Artist produces and encourage Him to do more: for He hasn't shown us a half of His capabilities yet. Oh the delights in store, the things God plans to do to us and for us! (The identification of the divine artist with the rich uncle is thus complete.) Here now are four illustrative quotations. The first is from the opening talk, which, after distinguishing between 'Biological life (or *Bios*)' and 'Spiritual life (or *Zoe*)', concludes as follows:

- (1) "A man who changed from having *Bios* to having *Zoe* would have gone through as big a change as a statue which changed from being a carved stone to being a real man.

And that is just precisely what Christianity is about. This world is a great sculptor's shop. We are the statues and there is a rumour going round the shop that some of us are some day going to come to life."
(p. 14.)

- (2) "What we have been told is how *we* can be drawn into Christ — can become part of that wonderful present which the young Prince of the universe wants to offer to His Father — that present which is Himself and therefore us *in* Him. It is the only thing we were made for. And there are strange, exciting hints in the Bible that when *we* are drawn in, a great many other things in Nature will begin to come right. The bad dream will be over: it will be *morning*." (p. 44.)

The second quotation was from a talk under the heading *Is Christianity Hard or Easy?* I give next the conclusion to a talk on the command 'Be ye perfect', which I think is particularly revealing:

- (3) "The command *Be ye perfect* is not idealistic gas. Nor is it a command to do the impossible. He is going to make us into creatures that *can* obey that command. He said (in the Bible) that we were 'gods' and He is going to make good His words. If we let Him — for we *can* prevent Him, if we choose — he will make the feeblest and filthiest of us into a god or goddess, a dazzling, radiant, immortal creature, pulsating all through with such energy³ and joy and wisdom and love as we cannot now imagine, a bright stainless mirror which reflects back to God *perfectly* (though, of course, on a smaller scale) His own boundless power⁴ and delight and goodness. The process will be long and in parts very painful; but that is what we are in for. Nothing less. He meant what He said." (p. 48.)

Lastly, a couple of irresistible excerpts from the final talk of the series:

- (4) "Well, the thing has happened: the new step has been taken and is being taken. Already the new men are dotted here and there all over the earth. Some, as I admitted in the last talk, are still hardly recognisable: but others can be recognised. Every now and then one meets them. Their very voices and faces are different from ours; stronger, quieter, happier, more radiant. They begin where most of us leave off." (p. 61.)

"When you have recognised one of them, you will recognise the next one much more easily. And I strongly

³ Note the word order here; ⁴ and here.

suspect (but how should I know?) that they recognise one another immediately and infallibly, across every barrier of colour, sex, class, age, and even of creeds. In that way, to become holy is rather like joining a secret society. To put it at its very lowest, it must be great fun." (p. 61-2.)

I think we can safely raise our hats to C. S. Lewis for having put it — all of it — at its very lowest and take a more favourable view in retrospect of Papini.

Turning again to the quasi-scientific element in theological discourse, we may now find it illuminating to reconsider this as a special instance of the quasi-aesthetic misunderstanding of religious discourse. To the quasi-scientific category belong all those discussions which are marked by a preoccupation with the *existence* of God and hence by speculation as to where or when God might exist and what sort of an existence God might have. There is above all a concern about *evidence* or *proof*, together with an insistence upon our knowledge or experience (absence of knowledge, lack of experience) of God and a desire to display belief in God—here construed as the belief *that* God exists—as rational (irrational) and resting on reasoned argument (inconclusive, mistaken argument). In general the discourse proceeds, or tries to proceed, as if the subject under discussion were a nebula or a secret agent—from the antitheistic theologian's point of view as if the subject were something scientifically disreputable, such as phlogiston or flying saucers, or an alleged secret agent whom investigations by the bureau of counter-espionage demonstrate to be an illusion. There is pretension to the use of scientific criteria, in spite of the fact that there is no scope at all for the development of techniques of measurement and calculation, no apparatus to build, no predicting, no possibility of arranging an experiment. The spirit of enquiry, which is akin to the spirit of aesthetic contemplation in any case, can only exercise itself in the circumstances as a kind of passive and playful conceptual picture-painting. Hence in all the familiar views about the evidence that is to be found in the universe for God's existence the argument is curiously vacuous, the evidence curiously tenuous. All we really get is just a view—an immediate quasi-aesthetic judgement. The world is a garden cultivated by an invisible gardener. A divine purpose is evident in the evolution of species (alternatively plagues and famines, volcanic eruptions and cancers demonstrate an absence of divine purpose). The universe is a watch (Butler) with God as the watchmaker. It is a mathematical system with God as the conceiver (Sir James

Jeans: "from the intrinsic evidence of his creation, the Great Architect of the Universe begins to appear as a pure mathematician . . . the universe begins to look more like a great thought than a great machine"). This brand of theology approaches its apocolocytosis in some recent writings of Oxford's Rouse Ball Professor of Applied Mathematics:

"Science itself must be a religious activity . . . science is one aspect of God's presence, and scientists therefore part of the company of His heralds . . . I want to be able to look at science, its methods, its presuppositions, its basis, its splendid successes and its austere discipline; and then I want to be able to say: Here is God revealing Himself for those with eyes to see." (C. A. Coulson, *Science and Christian Belief*, 1954, p. 30.)

"When Huxley and Darwin try to interpret whole realms of biological activity under the title 'struggle for existence', we should be willing to recognise kinship with at least one of the main themes of religious thought. When J. Z. Young speaks of man as made for coöperation and communication with his fellows, he is glimpsing something of the Christian experience of fellowship and the Christian doctrine of Heaven. When Jeans writes of the 'Mysterious Universe', and when Fred Hoyle speaks of its 'fineness, both in concept and design', they are not far from the Kingdom." (Ib., p. 89-90.)

The words "glimpsing something of the Christian experience" suggest a confusion here between the religious question of the nearness of Huxley, Darwin, Jeans, Young and Hoyle to God's kingdom (to which the religious response would presumably be that only they themselves severally—and God—might judge of this) and the conceptual question of whether or not they came near to referring to it at all in certain of their dicta. Not that the second question any more than the first can have been raised by Professor Coulson with any clarity; for his answer shows, in conjunction with his previously stated view that God is revealed in science, that he has himself mislocated this kingdom. There are many possible objects of study and aesthetic enjoyment, but God is not one of them. Professor Coulson misrepresents the situation as if it were that by comparison with such objects God is unique in being supremely the most subtle, great, splendid, elusive to the grasp; so that God reveals himself, not indeed to all scientists, but only to the discerning ones.

However, if Professor Coulson is far away (in the conceptual sense) from the kingdom of God then he is scarcely

further, and for fundamentally no different reason, than are those contemporary philosophers who conceive the problem of religion in terms of the problem of the invisible gardener. This particular hare was started by Professor Wisdom (*Gods*, 1944) and it has been cherished now for over a decade in North Staffordshire, Oxford and other places—apparently without its keepers perceiving that it was suffering from conceptual myxomatosis. In calling Professor Wisdom's article "haunting and revelatory" (*New Essays in Philosophical Theology*, p. 96), Professor Flew puts his finger unwittingly upon the respect in which its influence has been baneful. "Haunting" is the *mot juste* because there is a good deal in the article that is suggestive of Borley Rectory. "The feeling that the world is haunted—this is the feeling of those who believe in gods" (p. 164 of the reprint in *Philosophy and Psycho-Analysis*). I agree; and I call this feeling a superstition. Many religious people have it: but they are then superstitious as well as religious. The distinction between religion and superstition, which is of vital importance if religion is to be taken seriously, gets blurred in Professor Wisdom's article—largely, I think, because he is content to speak of gods and of God indiscriminately. "What are the stories of the gods? What are our feelings when we believe in God? They are feelings of awe before power, dread of the thunderbolts of Zeus . . ." (p. 164). The article actually begins with the following headline: "*The existence of God is not an experimental issue in the way it once was.*" To which one is inclined to reply that it *never* was except for those who misunderstood it; that anyone who has ever tried to treat it as such was *ipso facto* concerning himself with something other than God and religion. The trouble with the analogy of the invisible gardener is that, although it is intended by Professor Wisdom to illustrate a transition of the issue from an experimental one into one of quite another kind, yet in a curious way it preserves intact the essence of this misunderstanding. For it displays God as ceasing to be an *experimental* hypothesis but still remaining a hypothesis.

"When after all this, one says 'I still believe a gardener comes' while the other says 'I don't' their different words now reflect no difference as to what they have found in the garden, no difference as to what they would find in the garden if they looked further and no difference about how fast untended gardens fall into disorder. At this stage, in this context, the gardener *hypothesis* has ceased to be experimental . . ." (p. 155. My italics.)

Αὐτὸς ἔφη. And I suppose that *gods* indeed are 'non-experimental hypotheses'. That is why they are vain things which it is a confusion to believe in—the sort of confusion which can also be called superstition. Professor Wisdom's treatment of belief in God as a special instance of belief in gods involves the characterization of religion as a special case of superstition. Once a philosopher has done this he only makes matters worse if, in thinking to allow for the non-triviality of religion, he falls over backwards to show that superstitions can be serious things.

A sizeable contribution to quasi-aesthetic theology is made by those discussions in which it is supposed that God can be discovered or met with directly in a special experience called 'religious experience'. This experience, with the 'particular knowledge' of God it allegedly contains, is held by some to be absolutely essential to religion, since without it religion would have no sure anchor among the realities of human awareness. Evidently this is Professor H. D. Lewis's view:

"Particular knowledge of God religion must have, the more enlightened as much as any. But how is this possible if God is also so remote and so far beyond our comprehension as religion itself, as well as philosophical thought about it, declares Him to be?

It is for this dilemma that we must seek a solution in terms of a special sort of experience." (*Aristotelian Society Supplementary Proceedings*, 1955, p. 81.)

Professor Lewis would here seem to be solving his dilemma only by the Nelsonian expedient of causing one horn of it to disappear from view. Not that this prevents him from shipwrecking himself upon the other horn—which is what comes of engaging the wrong dilemma. For if, of any experiences which any persons may have had or may now have, there is to be sense in saying, "these are experiences of God", then we must be able to raise and have means of settling the question: When, under what conditions and in respect of whom will it be correct to say this? Now there is some hint, in the paper from which I have quoted, that Professor Lewis regards his postulated experience as a self-guaranteeing phenomenon—so that for those who have it the experience bears its authenticity upon its face. This notion, however, leaves entirely unexplained what it is for an experience to have religious significance at all. To be sure, it creates the illusion of an explanation, by suggesting that the religious significance of an experience can be located simply in its own unique and striking quality as an experience. But let your experience be as powerful as you please, let the experiencer have

much or little to say about its quality or its content, the question remains: What could entitle an experience, special or otherwise, with or without angelic forms, sounding trumpets, rushing wings, a booming voice, 'annihilation of self', or perfect peace—what could entitle *any* experience to be called an experience of God?

At one point in his paper Professor Lewis lets fall the following observation:

"There are men who claim to live continuously under the guidance of God. Such claims are not always convincing, and it is well known how prone certain types of religious people are to delude themselves or to adhere stubbornly to fixed ideas whose religious credentials are very doubtful." (*Ibid.*, p. 80.)

The problem is thereupon dismissed with the evasive comment, "This is a subject of study in itself". But even as it stands, the observation should have cast doubt upon the capacity of an *experience* to constitute the link between a man and his deity. How might it be possible for some people to be—as Professor Lewis here suggests—religious *in spite of* having delusive ideas or delusive experiences? And might it not be recognized that some people who make 'religious' claims concerning their experiences are not in fact religious at all? It is imaginable, for instance, that the custodians of Belsen should have had 'special' experiences which they described (and without the least intention of misreporting them) in terms which squared well enough with those used by saints and Christian mystics: they might conceivably have regarded these experiences as potent sources of inspiration, so that they were enabled to torture and massacre their prisoners with increased gusto. We should not suppose that they might through such experiences have come into any relationship with God; and the reason, I should think, would be obvious. Their entire way of life would preclude this possibility. Perhaps it might still be contended that *some* people at least have had experiences which were in themselves indubitable experiences of God—St. Paul, for example. But again, if St. Paul's experience is to be accounted an experience of God—I say *if*, because I do not want to suggest that we are obliged to say of St. Paul that he had a particular experience of God and I do not think we should be slighting him by declining to say this—then it will not be because of what we know to have happened on the road to Damascus (what *do* we know of this?), but only in relation to his subsequent life. If after his experience he had continued to live as he had lived before, then the experience would have been shown not to have been an experience of

God, would have been shown not to have had anything remotely to do with God, no matter what St. Paul himself might have chosen to say about it either at the time or later. If anyone should protest that it is unthinkable that St. Paul's life should not have changed in view of the nature of the experience he then had, I should reply that if this is not nonsense it is to reiterate what I have just said, only misleadingly and the other way round.

To insist, as Professor H. D. Lewis does, on the "remoteness" of God and on the importance of those traditional utterances which stress God's inscrutability (the God Who "hides Himself", Whose ways "are not our ways"; "no man hath seen God", etc. *Ibid.*, p. 73) and at the same time to insist upon a direct awareness of God ("immediate contact", p. 78) is surely to propound a contradiction. Those utterances, one must conclude, have not been understood and God is being regarded, in spite of disavowals, as a kind of celestial Lloyd George. However, the idea of a self-contained experience within which God could be directly known would still be a radical misunderstanding even if God *were* Lloyd George.

C. B. Martin, who has discussed the claim to experience God in an article called *A Religious Way of Knowing* (*Mind*, 1952; reprinted in *New Essays in Philosophical Theology*), believes that there is sense in this claim. It is significant that he does so in the light, or rather in the gloom, of an analogy with *aesthetic* experience. As his title intimates, Martin confuses religious statements with theological statements, and being unprepared, apparently, to discover error in the logic of statements about 'knowing God' in the sense of being 'confronted with God's personal Presence', he mistakenly fosters the idea that there is something "intricate", mysterious and unique about it ("It is not my wish to go from a correct suggestion that the logic is *very, very* like to an incorrect suggestion that the logic is *just* like"). But the logic of these theological statements does not resemble the "ineffable, effable, Effanineffable Deep and inscrutable singular Name" of Eliot's *Practical Cats*. It is just downright confused.

Knowledge of God is an expression that unquestionably has a place in religious discourse as well as in theological discourse, unlike such epistemological notions as *evidence*, *hypothesis*, *demonstration* and *proof*, which, when employed in relation to God, have THEOLOGICAL written all over them. Even the idea of *seeing God* is, or at least can be, religious. "Blessed are the pure in heart for they shall see God." This does not mean, "Blessed are the pure in heart for they shall have special experiences with a cognitive content". Nor would it be anything

other than foolishness to add, as if for the sake of completeness, "Blessed are the scientists too for they shall see God during the course of their investigations". *Religiously* speaking, the knowledge of God, or in this variant form the sight of God, is related to the love of God, to the sinner's repentance and his striving to do God's will, somewhat in the way that ethically speaking knowledge is related to virtue—as by Socrates when he said that virtue *was* knowledge; and Socrates did not mean either that the possibility of virtue depended on what kind or what amount of experience, special or otherwise, a person might have acquired, or that upon the attainment of virtue there supervened the knowledge of something else, something to one's gratification or advantage perhaps.

Evidence, hypothesis, demonstration and proof are linked (in theology) with the notion of God's *existence*. But knowledge of God is not in religious discourse synonymous with the knowledge that God exists, and though it might be supposed that it must at least *include* the knowledge that God exists I think this supposition must be a misunderstanding. The idea of God's existence may enter into religious discourse when a religious person asserts that God exists as a way of affirming his religious belief, his belief *in* God. But if in making this assertion he takes himself to be saying, not simply *that* he believes in God, but *what* he believes in regard to God (*i.e.* that God is a being who exists) then I think he passes at once from religious discourse into theology; he has started to talk *about* God, and in a quasi-scientific manner.

You may say, "But *God exists* must mean more than *I believe in God*, for God would exist whether I believed in Him or not". Certainly, a religious person must think that if he did not believe in God he would be making the most terrible mistake. But he ought not to suppose that he can indicate what this mistake is by saying that it is the mistake of believing that a being does not exist when really a being does exist. For what would be so terrible about that? And what *would* he be believing when he believed that this being (*Which* being? *Which what?*) existed? Professor H. D. Lewis recommends "the advocate of religion", in facing this question, not to be too anxious to forgo the embarrassment—as he puts it—of seeming "very evasive" (*op. cit.*, p. 73). What makes this recommendation seem partly right is that to ask what sort of an existent is being believed to exist when it is believed that God exists is to demand a scientific (or quasi-scientific) elucidation of the notion of God's existence. This inappropriate and logically unsatisfiable demand must certainly be evaded by the advocate of religion: *rejected* would be

the better word, and there is no need for embarrassment. Professor Lewis's embarrassment springs from the fact that the demand has been expressly made appropriate by his own presentation of God's existence as embodying a kind of factual hypothesis. He would like to treat God's existence as a hypothesis of *some* kind, though not one of any *specifiable* kind. The hypothesis is thus doomed to vacuity.⁵ But even if it were not, my point is that religion should appropriate to itself the words of Laplace and say that it has no need of this hypothesis. A *deus ex machina* (which is what this amounts to) is no less detrimental to religion than it is to astronomy.

Suppose that to have knowledge of God *did* mean to have come to be aware that a 'being' were somehow 'there': suppose that there *were* sense in this. Then it would be possible for a person to become religious simply by being presented with an item of information. Construed in this way, knowledge of God would render religious belief superfluous—would involve a positive cessation of faith, since it would replace it by a take-it-or-leave-it acceptance of fact. It is, to be sure, quite usual for theologians to insist simultaneously upon the possibility of this sort of knowledge of God and also upon the necessity of faith—a muddle which has expressed itself traditionally in the self-contradictory concept of a *necessary being*. Come what may, the faithful believer must believe in God, no matter what the world is like, has been like or will be like. God as an object of belief for him is necessary, not contingent; and so it may come to be said that God is a necessary being. But the greater this necessity of God the remoter and increasingly dispensable becomes the being. Until if there is to be room for an infinite faith—and it is by its infinitude that the faith of religion distinguishes itself from pagan faiths, it is as an object worthy of an infinite faith that the God of religion is distinguished from pagan gods—then there cannot exist *even the possibility* of a "cognitive encounter" with the object of that faith: which is to say that the object of that faith cannot possibly be any sort of existent. To worship an existing thing, howsoever remarkable a thing one imagines it to be, is idolatry. To have religious faith in a fact is a contradiction.

The necessity of God is not the necessity that God should be (be a fact), but rather the necessity that God should be what He is, *i.e.* that He should possess *in aeternum* the properties which are ascribable to Him. God's infinity relates to the manner in which He should possess these properties: it is not an infinity

⁵ It becomes, in fact, the superstitious non-experimental hypothesis which Professor Wisdom seemed to confuse with religion in *Gods*.

of existence—not for instance of vastness or location or temporal span (whatever such a brand of infinity be supposed to signify); and not of grandeur either, for to call God infinite from an idea of what is fitting to ‘His Majesty’ is a quasi-aesthetic corruption. Those congregations who are enjoined regularly to “Worship the King all glorious above” would say, I suppose, that they regarded the deification of kings, the emperor cult of the Roman Empire for example, as superstition. But if the deification of kings is superstition, so also is the kingification of God.

“Nevertheless, when we pray, we must be praying to *someone*, praising *someone*, asking forgiveness of *someone*, asking help of *someone*: otherwise our words would be empty and worship would lose its sense. Not that we thereby treat God as any ordinary being, not indeed like a king, for in religion the reverence accorded to God is an exceedingly special reverence which is due to Him alone.” But I ask, *what* sense would worship lose? For if it cannot become clear to anyone what the sense of worship is without it being said that God is a being (an existent), then I do not see how it can become clear when this *is* said, since the sense of the assertion that God is a being is itself, to say the least, *unclear*. If you attempt to explain whatever is special about the reverence that is due to God alone by saying that it is the reverence which is appropriate only to the special being that God is, you merely saddle yourself with two kinds of specialness, which either drift together freely in a void or else get anchored in some artistic formula. Such a theological technicality as for instance the distinction between *δουλεία* and *λατρεία* seems either to tell you nothing at all or else something like this: that in the presence of important persons you should go down on one knee, but in the presence of God you should go down on two. As for petitions to God, their proper form is always, “Thy will be done”. So that the petitioner is constrained to ask for whatever he is going to get; which in a way is the same as asking for nothing—asking in such a way that it should cease to be of interest to postulate a dignitary who might function as donor. The essential point about “Thy will be done” is that it is emphatically *not my* will that is to be done.

Fear may arise on the part of a religious person lest in his spiritual exercises his practice should become indistinguishable from that of a devotee of M. Coué’s ‘practical psychology’. One man says, “O God, make me good”; another says, “Every day I’m getting better and better”. The difference between them could indeed be trifling: or it could be colossal. But the religious person, whose practice it is to worship, will only delude himself

by imagining that the distinction might be guaranteed by the introduction of a special existent. For what difference would this addition make to the character of the practice itself? In one way it might affect it, I suppose, but this would not be as the moon affects the tides (superstitious thought!) and it would be the reverse of the effect that was desired. For it might assist the degeneration of worship into a species of opportunism: by reiterating to himself that in praising, thanking and making petitions to God he is praising, thanking and making petitions to *someone*, the worshipper might be encouraged to utter his praises as if they were congratulations, to give his thanks as if he were acknowledging a good turn, to make his petitions as if he were craving a *pourboire* or applying to a county court.

With regard to such religious statements as that God is good or that God is merciful, I have suggested that the sort of goodness and the sort of mercy (namely, *infinite* goodness and *infinite* mercy) which theology itself wishes to predicate of God, and also the manner in which it wishes them to be predicated (namely, as *necessary* attributes of God), are precisely such as to preclude them from being predicated of any existent. The assertion that God created the world might be thought to be in a special position and to involve a reference to an existent (the Creator) even if these other assertions do not. But this too is an illusion. For by "the world" is meant "all that there is", and no existent could sensibly be said to have created all that there is because it would itself have formed a part of what there is; besides, there would only arise in turn the question of its own creation. What makes it possible for the assertion that God created the world to have (religious) truth is not that some big cosmological event should have taken place, but that this assertion should be uttered by one who is grateful for his existence and finds it good. God's goodness and mercy are thus linked with His creatorship internally (the linkage is one of meaning: a quasi-scientific or quasi-aesthetic creator could well have been neutral or bad).

To say that God created the world is like saying, "I owe my existence to God", which in turn is like saying, "I do not owe my existence to myself". This—creation by God—is the beginning, not as the history of Rome might be said to have begun with the founding of the city by Romulus, which is a typically mythological account of the way a chain of events got started, but in the sense that it is the foundation of the religious person's attitude towards existence.

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INDUCTIVE PROCESS AND INDUCTIVE INFERENCE

By G. BUCHDAHL

It is well known how frequently we find philosophers or non-philosophers wondering whether one can (or "really" can) argue from the known to the unknown, from the past to the future, from evidence supporting a law to the law itself. Familiar also are the differences in viewpoint which soon emerge and the sorts of argument urged in their favour. Some will say that we have discovered large numbers of scientific laws; some that we have not or that we cannot be certain of this or that we can only derive conclusions that are probable.

In looking at these arguments again I want to do several things. I want to suggest that the differences just remarked on derive in part from the way in which writers choose to formulate the process of inductive reasoning. It will be found that some of these formulations apparently whittle away certain logical elements (and with this, some of the usual "difficulties"), elements on which others place what may perhaps be an undue emphasis. By drawing attention to these contrasts it is possible to place certain features of inductive reasoning in a different perspective. (In this connection, some attention will be paid to Mill's position, which will be seen in a new light.) I then want to show that it is odd to come down on one side of the fence rather than another. Though it is queer to say that Nature is uniform, it is equally odd to say sharply and firmly that inductive argument is possible without this assumption.

I

We find in the literature a number of references to the so-called "Problem of Induction" which indicate the existence of different "moods". First, I want to give an example of what I will call the "metaphysical mood" or M-mood. Max Planck, in *The Universe in the Light of Modern Physics*, says:

"How do we discover the individual laws of physics, and what is their nature? It should be remarked, to begin with, that we have no right to assume that any physical laws exist, or if they have existed up to now that they will continue to exist in a similar manner in future."¹

¹ M. Planck, *The Universe in the Light of Modern Physics*, 1931, p. 63.

In a similar mood, Hume, in the *Treatise*, writes:

"Anything may produce anything . . . (and) 'tis possible for all objects to become causes or effects to each other" ²

And as recently as 1948, Hempel and Oppenheim wrote (in their well-known article "The Logic of Explanation"):

"A given empirical statement S can never be definitely known to be a law." ³

I need not consider this position in any detail. But there does seem to be an argument in its favour which, though having the appearance of a joke, is none the less very powerful. We are dealing here, it may be said, with ampliative inference, i.e. inference from the known to the unknown. Hence nothing prevents us from arguing that, though all known ravens are black, we have absolutely *no reason* for inferring that the unknown ones will be black also (nor, of course, that they will be—or will perhaps be—*not* black).

This argument shocks us. It reminds us that the sort of considerations that make for "Leading Principles" cannot be of the sort we invoke when we reason in accordance with them. Moreover, it will clearly be difficult to *argue* anyone out of such a position if he chooses to adopt it. This is supported by the fact that philosophers, when confronted with it, begin to look at the whole inference situation in a different way. They adopt different moods. For instance, C. A. Mace, in his *Principles of Logic*, like Planck, finds himself unable to discover any non-impugnable logical basis for the assumption that physical laws exist. However, he then goes on to say:

"That there are such laws . . . would seem to constitute the most tangible basis of our assumption that induction is somehow valid." ⁴

Quite clearly, the author is now thinking in a different mood. I will call this the "procedural or phenomenalist mood" or P-mood. Of course, one who persists in an M-mood will demur that it is an open question whether we have discovered any "laws" in the required sense.

² D. Hume, *A Treatise of Human Nature*, ed. Selby-Bigge, 1946, p. 173.

³ C. G. Hempel and P. Oppenheim, "The Logic of Explanation", in *Readings in the Philosophy of Science*, 1953, p. 338.

⁴ C. A. Mace, *The Principles of Logic*, 1933, p. 380.

We meet the P-mood in Hume. Thus he says in the *Treatise* that by inductive proofs he proposes to mean:

"Those arguments, which are deriv'd from the relation of cause and effect, and which are entirely free from doubt and uncertainty."⁵

Though "anything may produce anything", there is some sense in which we must allow a species of proof that can be "entirely free from doubt and uncertainty".

Of course, such moods do not usually remain standing side by side without some attempt at mediation. To this end, philosophers frequently look at P-mood statements as linguistic recommendations of some kind. Mace, for instance, might have replied to his M-mood objector that by laws (in the passage referred to) he meant only what men have conventionally agreed to call "laws of nature". Hume once more provides the best example. For immediately after writing "since therefore 'tis possible for all objects to become causes or effects of each other" he continues: "it may be proper to *fix* some general rules, by which we may know when they *really* are so".⁶ And elsewhere, instead of saying that one must "fix" such rules, he says that it is by such general rules that "we *ought* to regulate our judgment concerning causes and effects".⁷

All this might give the impression that Hume was *laying down* when one was to *speak* of causes and effects and correspondingly under what circumstances one was to be permitted to say that one was "really" dealing with a law. Of course, when Hume fixes rules determining when objects *really* are causes, he doesn't write "really" in quotation marks. Yet his remark might be taken to function as a linguistic rule (let us call it a statement in the L-mood). Hume, I think, wavers here between a P-mood and an L-mood. Generally, an M-mood may cause a P-mood to be converted into an L-mood. The L-mood mediates between P- and M-moods. Thus Hume writes:

"One would appear ridiculous, who wou'd say, that 'tis only probable the sun will rise to-morrow, or that all men must dye. It is . . . more convenient . . . to preserve the common signification of words."⁸

⁵ Hume, *op. cit.*, p. 124.

⁶ These general rules being instruments by which "we learn to distinguish the accidental circumstances from the efficacious causes". They turn out to be a rough anticipation of Mill's Canons.

⁷ *Ibid.*, p. 149.

⁸ *Ibid.*, p. 124.

One may wonder whether these different moods, with their changing ways of speaking, are not suggestive of different theories of Induction, owing to the fact that they will lead to a variety of characterisations of inductive reasoning each claiming to be "natural". This would explain both the existence as well as the persistence of such theories; but it would also make us suspicious of the basis of their arguments. This I will now discuss through examples of characterisations of inductions that have actually been put forward.

The choice of examples is, of course, a delicate matter. The shocker concerning a raven's colour suggested that inductive reasoning was a sort of stepping from evidence to conclusion. To this a critic might object by asserting that there isn't anything in scientific reasoning proper corresponding to such a step. And he would support this by reminding us of what we would say: for instance, that in science one collects evidence supporting a hypothesis; or that a scientific law is not a summary of observations, nor a deduction from them. Rather, all we have is the fact that certain recorded evidence indicates how the phenomena conform to the law. This characterisation would not be of much use for my purpose, for it is neutral to the question of the sense in which one is justified in speaking of "conforming" and of "law". The difficulty of Philosophy is surely that whilst much of our business arises out of the way in which certain problems are formulated and discussed, not every formulation is acceptable. We cannot gain light from an uncritical collection of case-studies of "what people would say" on a given occasion.

II

Let me therefore ask again: is there any element in inductive reasoning one would wish to *describe* as a "step"? I want to show that there are characterisations of induction which suggest for an answer a clear "yes"; others, an equally clear "no". Of course, none of these characterisations are forced upon us; unless we *adopt* one or the other of them, a philosophical problem doesn't arise.

Consider the following. Newton, in the *Opticks*, Qu. 31, says:

"Analysis consists in making experiments and observations, and in drawing general conclusion from them by induction."

* H. S. Thayer (ed.), *Newton's Philosophy of Nature*, 1953, p. 178.

And in a letter to Cotes he writes: "In this philosophy, propositions are deduced from phenomena, and afterwards made general by induction."¹⁰

Here is a hint how the suggestion for a "stepping process" may arise. The first part of the passage from the letter to Cotes, despite the use of the word "deduce", clearly refers to those operations or processes by which certain laws, or expressions intended to function in this way, are formulated. To whatever comes under these operations I propose to give the name "Inductive Process" (I.P.). Sometimes this may literally involve something like deduction, e.g. when Newton derived the inverse square law from Kepler's laws together with his laws of motion; mostly, however, it is something else, as I shall show in a moment.

The second part of Newton's remark seems to refer to something other than the I.P., to something that is done "afterwards"; i.e. "making general" the original expression, or "drawing a general conclusion by induction". This aspect I propose to call "Inductive Inference" (I.I.).

The distinction between I.P. and I.I. is (I hold) of the utmost importance. One may (at least implicitly) also find it in Mill. Mill defines induction as that which

"consists in inferring from some individual instances in which a phenomenon is observed to occur that it occurs in all instances of a certain class, namely, in all which resemble the former in what are regarded as the material circumstances".

He adds:

"In what way the material circumstances are to be distinguished from those which are immaterial . . . remains to be pointed out."¹¹

This is clearly a reference to his so-called "Canons of Induction". These also are (perhaps rather crude) instances of what I have called the I.P. whereas the first part of Mill's definition (speaking of an inference from some to all members of a given class) refers to the I.I.

An example using Mill's letters may be helpful, though exaggeratedly crude. Suppose we have instances XaY , XbY ,

¹⁰ Ibid., p. 7.

¹¹ E. Nagel (ed.), *John Stuart Mill's Philosophy of Scientific Method*, 1950, p. 181.

XcY . . . then one of Mill's Canons determines that we shall say that $a, b, c, \dots$ were irrelevant, and the Y 's agree in being here associated with X . Thus far the I.P. (we sometimes even put this by saying: *therefore* in the sample the Y 's agree in being associated with X). The I.I. consists in inferring that *therefore* the Y 's are associated with X "invariably and unconditionally" (as Mill puts it)—excepting of course such limiting conditions as are either explicitly or implicitly associated with the law.

The contrast between I.I. and I.P. focuses our attention on a peculiarity in Mill's account, indicating that he himself was not always clearly aware of the distinction. Apart from considerations of psychology, the need for a multitude of instances arises solely for the I.P., through which we determine what are the material circumstances. Once this is accomplished, it is hardly necessary to speak of inferring a general conclusion from *some* individual instances: indeed, Mill on occasion says that in such cases one is as good as a thousand, e.g. one measurement of specific gravity of a metal. On the other hand, there does follow from this distinction another rather important consequence for our evaluation of a theory such as Mill's. For since the I.P. in effect amounts to an account of what scientists usually do in arriving at the statement of laws, it follows that Mill's account of I.I. is not refuted as such by showing that his account of the I.P. is rather specialised or unworkable, or outmoded. Nor is his account of I.I. (i.e. the *logical* question) refuted by the consideration that scientists do not usually "generalise over particular instances".

Some further remarks about the I.P. may here be called for. Through it one determines in physics the scope and form of a law. And here Mill's Canons are indeed rather out of place. But then Mill, in his controversy with Whewell, especially remarks that in physics the "method of hypothesis" is more apposite. Indeed, nothing in an account such as Mill's is affected by allowing for the whole complicated apparatus by which one formulates and tests a scientific theory. Moreover, this is a temporal process. A theory doesn't become adequate all at once; it may fail at various stages and have to be modified. Thus Newton, after the initial deduction of the inverse-square law from Kepler's laws, uses certain additional values of the earth's diameter (at first incorrect, later replaced by the correct one) and extends his calculations thereby to include the moon's motion. And the I.P. need not and cannot stop at this point. It will have

to extend further, to the stellar system, using additional information and calculations.

The I.P., involving a method of procedure taking place in time, is thus a constant and continuous attempt at adapting some suggested account to the facts. Fresh evidence may for ever pour in on it. To adapt an expression from Waismann, the process has an "open texture". Can we say that the I.P. is ever complete? And until it is complete, can I proceed to the next step, that of I.I.? Newton's and Mill's accounts, so far, leave this question open; and rather than discuss it, for the moment I want to ask a different question. If my account of the I.P. is correct, what is the *function* of I.I.? Just what did Newton *do* when he spoke of "making general by induction"? And what was there to "infer" for Mill, apart from the I.P.? For instance, suppose I have "deduced" the inverse square formula as described; I thereupon subject it to a new test case, e.g. the moon's motion; I subsequently extend it further to the stellar system: what more remains but to write down my formula? Or, to use Mill's framework, once I have made sure what are the *bona fide* members belonging to the class of some specific metal, through a judicious process of elimination of forgeries and similarly complicating factors, what more remains than to write down the result of a specific gravity measurement in the form: The specific gravity of this is such and such?

Now I know what one wants to reply; for Mill's account of induction strongly suggests an answer. What we want to say is that we take a further step, that we pass from that last measurement to an open class of further measurements as yet to be made. Now this passage amounts to an inference, an inference for which a justification is still being demanded. But what does the inference amount to? What has been done when this inference has been made? For we now come back to the fact that our I.P. has an open texture. Schlick once put this very appositely by saying that the formulation of a law is arrived at with the "implicit proviso to modify it on the basis of later experiences"¹²—clearly a reference to our I.P. In a sense, so we might say, the I.P. is unending, and talk of generalisation, let alone "inductive step", only a sham, or linguistic convenience. It is only Mill's formulation that has misled us. We are strongly tempted to look in the direction of a formulation which will be more forthright, closer to the facts, less likely to commit us to such questions as "what justifies the inference?".

¹² M. Schlick, *Gesetz, Kausalität und Wahrscheinlichkeit*, 1948, p. 22.

III

Mill's Leading Principle was: Determine relevant conditions, then make an inference. Let us compare this with a different Leading Principle, mentioned in Newton's *Principia* as the so-called Third Rule of Reasoning.

"The qualities of bodies . . . which are found to belong to all bodies within the reach of our experiments, are to be *esteemed* the universal qualities of all bodies whatsoever."

And he adds in explanation:

"For since the qualities of bodies are only known to us by experiments, we are to *hold for* universal all such as universally agree with experiments."¹³ (*Italics mine.*)

With this I want to compare yet a third formulation, this time by a contemporary, E. A. Milne. Milne, when discussing the concept of "Law of Nature", characterises what he takes to be the schema of traditional induction as follows:

"When in the judgment of the observers or experimenters concerned . . . a sufficient amount of evidence has been secured (i.e. by variation of conditions) the regularity is *stated in the form* that such and such phenomena, qualitatively or quantitatively described, *always* accompany such and such other phenomena, and this is called 'law of nature'."¹⁴ (*Italics mine.*)

In my language: As the outcome of a sufficient application of the I.P., a certain regularity is *stated* (in some form or other).

Here then we have two other ways of talking about the principles of scientific reasoning which are alternatives to that discussed before. Mill had said: we *infer* a general conclusion. Newton (in this passage at least—he isn't very consistent) and Milne suggest a more positivistic approach. They remove the suggestion of a "step". Newton, by saying that *we are to hold*, i.e. treat, some given evidence in a universal way; Milne, that we in fact state something in a certain way, on the basis of something else called evidence. (Of course, these different formulations *need* not point to different theories of inductive inference. As I have remarked already, it is dangerous to overemphasise the implications of people's choice of terminology. Thus Newton

¹³ Thayer, *op. cit.*, p. 3.

¹⁴ E. A. Milne, *Modern Cosmology and the Christian Idea of God*, 1952, p. 11.

might claim that this prescriptive language was dictated by insight into nature's workings; whereas Mill might say that with his formulation he wished to do no more than give an account of what people actually do or say when reasoning inductively. On the other hand, it is possible that the differences of form to which I am drawing attention do hide bigger divergencies of viewpoint. And these in their turn may well receive some subtle support from the former.)

Let us see whether Newton's and Milne's formulations are free from difficulties, whether they avoid the earlier perplexities; and, if so, at what cost. To do this, I first want to deal with a minor stylistic objection. Milne says that a law states something in the form that certain phenomena *always* accompany certain others. It may be objected that this, too, is saying too much. In "all ravens are black" there is indeed explicit reference to an open class but most physical laws are not put in this form. For instance, Boyle's Law might be held to be completely expressed in the formula $p.v = k$. However, the formal expression of a law usually does not completely represent its physical or logical content. We have to ask how the expression is meant to function. We would like to say that it expresses some connection which holds always, or universally, or invariably and unconditionally, as Mill put it. Unfortunately, the precise sense of "universality" here involved is not clear but itself presupposes a clarification of the nature of inductive reasoning.

Perhaps we may get further by studying the genesis and first statement of this law in published form. Boyle wrote a small tract called *A Defence of the Physico-Mechanical Experiments, against the Objections of Franciscus Linus* To clinch his argument that it is air-pressure which supports a barometric mercury-column, he tries to show that the greater the pressure the larger the column it will support. Towards the end of the book this is put in quantitative form, by giving "A table of condensation of the air", the last column of which is headed: "What the pressure should be supposing it in reciprocal proportion to the expansion."¹⁵

Turning now to a comparison of our three formulations, we may ask: At what point would Mill pass from Boyle's "supposition" to an "inference"? Or, for the case of Newton and Milne, respectively: what happens between "supposing" that certain relations hold, and "holding" them or "stating" them to hold universally? Now Mill's talk of "inference" suggested that when

¹⁵ P. S. Shaw (ed.), *The Philosophical Works of the Hon. Robert Boyle*, vol. II, 1738, p. 671. This column of course agrees tolerably well with what purport to be the experimental readings.

you pass to a conclusion something important has happened to the original supposition. I think this is suggested by the fact that one seems almost inescapably driven to ask for a justification of this passage. One feels that we have arrived at something like "new knowledge", that we have discovered, so to speak, an additional proposition. On the other hand, the alternatives seem to imply that to accept a law such as Boyle's, after having secured "a sufficient amount of evidence" (i.e. after having completed the I.P.) amounts to no more than a *decision to use a certain expression for certain implicative purposes*. This interpretation harks back to Schlick, who wrote:

"A law of nature at bottom doesn't possess the logical character of a 'proposition' but rather represents a '*specification for the formation of propositions*'."¹⁰

Here, too, a decision language seems to be implied. The main aspect of this language is that it suggests that it is we who decide at some stage of the I.P. to express its result in more general form; and furthermore, that the expression of this result is subject to indefinite modification owing to the open texture of the I.P. In this way the decision language doesn't seem to run the risk of the earlier formulations, of committing itself to an assertion about the future, for instance. Nevertheless, there are difficulties. The concept of "decision" is hardly "at home" in these circumstances. To see this we need only consider cases where we decide literally to use some expression and retain it in the face of contrary evidence. Cases of statistical laws come to mind; normative statements, such as Ideal Gas Laws; the laws of motion. But when we want to give to inductive reasoning the *logical* character of a "decision", the common paradoxical features of a philosophical "reduction" in the positivist direction emerge. The proposed models never quite fit. Thus, in the light of the open texture proviso, it seems evident that whatever "decision" we make in such cases we aren't *committed* to anything, we issue no claims on the future.

On the contrary, as we saw, this language was invented to *remove* the suspicion that what Mill had called "inference" (i.e. generalisation) involved some special claims as to the future which we ought to make good; claims which in the light of the open texture of the I.P. might be held to mean very little.

Yet there is something rather peculiar about this language. Consider Newton's Leading Principle. When Newton said that one was to "hold universal what universally agrees with experi-

¹⁰ Schlick, *op. cit.*, p. 23.

ments", he might be thought to have made a certain procedural recommendation, on lines similar to those which before we encountered in Hume. The result of a given I.P., at some given point, is to be evaluated in a certain way, by proposing to use certain expressions for certain purposes, e.g. predictive ones.

However, this language has some odd features, as can easily be seen. If we take it seriously, we might say that Newton, for instance, will express the fact that all observed ravens have been black by the propositional specification "all ravens are black". However, such a "specification" is after all intended for certain purposes; it is meant to *give a reason* for saying that the next raven will be black. But does it make sense to speak of a certain *chosen way of speaking* as being given as a *reason* for making an inference, e.g. here: a prediction?

How could this difficulty have escaped us? There is one point worth noticing, which emerges from a careful comparison of Newton's and Milne's way of speaking. For while Newton demanded that "we hold something as universal", Milne merely said that we *in fact state* the result of an investigation in a certain form, implying universality. Milne thus describes the logical situation (I.I.) in its least septic form. He gives a procedural account of "logical behaviour" so to speak. We now see how this kind of language so strongly favours the P-mood, for it simply tells us that people in fact do express their conclusions (in certain ways) on the basis of certain empirical investigations. It is this language which favours views such as Waismann's, expressed as the doctrine that "the known relations of logic" do not hold between inductive evidence and conclusion.¹⁷

It seems then that the decision language is both too weak and too strong. It is too strong for it misleadingly suggests connections with actual decision situations, and it is too weak in a way which is brought out by Mill's counterformulation in terms of "inference". For this reminds us of the oddness of suggesting that we can simply lay down by a linguistic fiat that, given the black colour of known ravens, we are to *say* that the unknown ones are black also. Why not lay down the opposite rule? Surely something more must be involved here, something that moved Hume to speak of Leading Principles of this kind as

"principles which are permanent, irresistible, and universal . . . upon their removal human nature must immediately perish and go to ruin".¹⁸

¹⁷ F. Waismann, "Verifiability", in A. Flew (ed.), *Essays on Logic and Language*, First Series, 1951, p. 128.

¹⁸ Hume, *op. cit.*, p. 225.

IV

Behind the formulations of Mill, Newton and Milne lie those different moods of which I spoke in the beginning. But though the latter may supply the background and driving force for these formulations (and theories), it cannot be said for this reason that they have to be accepted at face value. On the contrary, each in some odd way seems incapable of fitting the required over-all features of the inductive situation. There is a further complication which in the long run, however, will help us to get greater clarity on the subject. I suggested that a part-motive for the decision language was an awareness of the open texture of the I.P. This is, however, misleading, for it is by no means impossible for a Mill-type of inference language to be associated with an open texture view. Newton himself, for instance, in his 4th Rule of Reasoning, writes:

"In experimental philosophy we are to look upon propositions *inferred by general induction* from phenomena as accurately or very nearly true . . . *till such time* as other phenomena occur by which they may either be made more accurate or liable to exceptions."¹⁹ (Italics mine.)

This clearly once more raises in an acute form the question of the relation between the I.P. and I.I. To study it, I want to consider some further aspects of Mill's doctrine which are fascinating if only because they show a fairly clear regard for the distinctions we have been making; and because they show why neither of the views discussed so far is a serious reply to the other.²⁰

Mill, in his controversy with Whewell, quite clearly distinguishes between I.I. and I.P. According to Whewell, Kepler's discovery of the laws that go by his name had been a supreme instance of induction. Kepler had brought a new conception to bear on the phenomena and thereby connected isolated facts (planetary positions) through certain quantitative expressions. Mill replied that all this was not induction, by which *he* meant here, I.I. There was hypothesis, he said; description; discovery, invention, imagination and — so he might have added — deter-

¹⁹ Thayer, *op. cit.*, p. 5.

²⁰ If in what follows I have not succeeded in interpreting the many conflicting strands in Mill's thought correctly, this will not, I hope, invalidate the argument. I shall be content to have used what appears to me a possible account of his views as a point of reference for discussion.

mination of the field of application. All this, however, he maintained, was not the core of induction.

"The only real induction concerned in the case consisted in inferring that because the observed places of Mars were correctly represented by points in an imaginary ellipse, therefore Mars would continue to revolve in that same ellipse."²¹

Put in other words, Mill was saying this: Certainly Kepler had attempted to give us an expression which was the most adequate possible, i.e. which best fitted all the known evidence, all the evidence one could possibly have wished for. And this, he wanted to say, is a remark about the result of an I.P. The real question, however, was this: will the law remain adequate? The making of an inference amounts to an assertion that it will remain so.

Now, in the light of the earlier discussions, we must, however, ask whether Mill's "real question" means anything at all? And I will first urge some considerations which suggest that it doesn't and afterwards some to the effect that it does—though it will be found that the meaning involved is in some sense "unclear", an expression of the metaphysical mood.

First, then, how *can* one attach any meaning to the question "will Mars continue to revolve" in the face of Schlick's and Newton's open texture proviso, i.e. the proviso which formulates the fact that it is the very essence of a law that it must be modifiable in the face of further (and hence, future) evidence? So Mill's question cannot mean: "Can you read the future?". Likewise, when he says that the question is, will the law hold invariably and unconditionally, he cannot mean: come what may. As S. Toulmin has pointed out, "saying a law holds universally is not the same as saying that it is true always and not only on certain conditions".²² But what else could Mill have meant?

Let me add a second consideration. To say that we make an inference to the effect that something is the case unconditionally and invariably (subject of course to *stated* conditions) suggests at least that we require a ground which would guarantee that we are entitled to draw such a conclusion. What most usually comes to mind is the assumption of the Uniformity of Nature. Yet, in the light of the open texture of the I.P., it is not clear what this could mean. For, as has been pointed out, could not the failure of a law to hold be simply evidence that our statement of it was inadequate rather than that nature was non-uniform?

²¹ Nagel, *op. cit.*, p. 177.

²² S. Toulmin, *The Philosophy of Science*, 1953, p. 80.

Now I think that it is quite possible that these objections still miss the distinction between I.I. and I.P.; and that a greater awareness of it led Mill to believe that some meaning could be attached to his "real question". For Mill, I think, was not concerned with the question, will and can a law become inadequate owing to fresh evidence coming forward as from now? (which would be a question concerning the I.P.). On the contrary, what he was asking was this: If at this moment I am in the possession of what may in any sense be called good and sufficient evidence am I then entitled to say (i.e. infer) that I have an adequate law? In other words, he was not interested in asking: Will my law remain adequate? but, is it adequate now?

A difficulty arises at once. In what sense is evidence here said to be "good and sufficient"? How could it be "sufficient", for instance, in the light of the open texture of the I.P.? I think, however, that the answer Mill would have to give to this question is quite clear: It is one thing to ask whether we do or do not possess good and sufficient evidence; but, granted that we do, are we *entitled even then* to assert the conclusion?

Is this an odd question? You may think it so odd as to feel that it does not require an answer. Nevertheless, people often feel just as strongly that it requires a very firm answer, namely, that from good reasons we *can* derive a conclusion. What could be more natural? And yet, what could be more odd? For is there any doubt?

To be sure, as we shall see in a moment, Mill answered the question in the affirmative also. My point is that for him there were two questions involved: one concerning the sufficiency of the evidence, the other the matter of drawing conclusions.

V

To conclude, I want to look at this matter from another angle by considering very briefly and in my own words certain aspects of Mill's theory of the "justification" of induction; a theory whose full subtlety it is very easy to miss. It seems that Mill in effect says this: We could not speak of ever having discovered any laws, i.e. of having come to any inductive conclusions, unless we assumed that nature was lawful. Now, we *discover* that nature is lawful, for we have discovered many of its laws. And this in turn, and finally, supports the assumption of lawfulness.²⁸

²⁸ Nagel, *op. cit.*, p. 287 ff.

The usual objection to this I noted above: Surely you could not speak of having discovered *any single* law, unless you first knew the general assumption to be true; without it you haven't *really* anything you are entitled to call "supporting evidence". And even if you had it, i.e. if you knew on independent grounds that you were in the possession of some *bona fide* laws (in the sense required), how could this fact even begin to contribute to your conclusion concerning the general lawfulness of nature, unless that principle were itself presupposed, i.e. unless you presuppose that inductive conclusions are ever valid?

Now to these questions Mill has an answer which is of the last importance in bringing out clearly what is involved. "Assuredly", he says,

"if induction . . . were an invalid process, no process grounded on it could be valid But though a valid process, it is a fallible one, and fallible in very different degrees."²⁴

In my language, he distinguishes here between the I.P., which is characterised by varying degrees of *fallibility*, and an I.I., in connection with which alone one can properly speak of validity. An important point should at once be noticed. For Mill the validity of I.I. cannot be "justified" by an appeal to the "uniformity of nature". On the contrary, it is what makes talk about uniformity possible. This shows how misplaced it is to ask for a further *support* in favour of what Mill calls the "validity" of I.I. And yet, at the same time, Mill (at least) does not think that the senselessness of *that* request supports the view that it is senseless to *ask* whether induction be valid.

This brings me to a second reflection. Mill's argument that we can in principle *discover* that nature is lawful is not dissimilar to the argument of those who hold that inductive methods are justified because they are successful. But it will be seen at once that this must be a remark about an I.P. For this is something that can *ex hypothesi* be discovered, whereas the *validity* of I.I. is *not* something you discover, but which according to Mill is presupposed. And it is clear that if the success-argument is to be more than a mere triviality, i.e. if it is to be a serious answer to Mill, then it must first be shown that it makes no sense to speak of "validity" in Mill's sense at all. Without that it is always open to an objector to reply to the argument from success by questioning the premisses, viz. *have* we been successful?

²⁴ Ibid., p. 290.

But why did Mill not think that it was senseless to ask or assert that induction was valid? Is anything substantial asserted by this at all—now that we realise that it means: Is I.I. valid? It was asked: I have good and sufficient evidence, but am I entitled to draw the conclusion? Suppose we talk in the language of Newton's 4th Rule, and say that we adhere to propositions inferred by general induction until they are disconfirmed; or in the language of E. A. Milne, to the effect that I.I. *means* stating the evidence in a certain form. I think in this language the possession of "good and sufficient evidence" would seem to *entail* that we are entitled to draw the conclusion.

But *must* "good and sufficient evidence" be defined in these terms? Could it not be defined solely in terms of the I.P.? Take Mill's example. Suppose Mars suddenly stopped revolving in an ellipse. I do not think that this fact by itself would show that Kepler's evidence had *not* been "good and sufficient".

Let me explain. We might say that when the planet stops revolving the law *had now become* inadequate; not that it *had been* inadequate when it was first formulated. But against this you might insist that it had always *been* inadequate because after all it did not predict correctly. As far as this goes, it suggests two different criteria for calling a law "adequate": (1) Whether it predicts; or more generally, whether it leads to consequences which follow from it and which did not form part of the basis on which at some given time it was formulated. (2) Whether at such time it was formulated in the best possible way, given all the evidence that I can be expected to have taken into account. The last-named alternative means that we tie "goodness and sufficiency" to the I.P. What shall we say about the first, the criterion of prediction?

We found that Mill had said, in my language, that the I.P. was to varying degrees fallible; but that I.I. was valid. Moreover, in one form or another he clearly tied I.I. to predictability. But let us ask: what would it be for a principle of I.I. to be invalid? Now unfortunately there is a sense in which we cannot show such a principle to be valid nor invalid, namely in the sense of "leading to right (or wrong) predictions". We cannot show this because, as we have seen, in I.I. we are *always* concerned with the conclusions we may draw from given evidence *at any given time*. There is a temptation to say: we *can* show a certain given leading principle, a certain type of inference, to be invalid, since it constantly leads to false predictions. But this would not be a consideration at all. For this would be a remark about the I.P. and not I.I. It is for this reason that all those

discussions which make the justification of induction hinge (in one way or another) on some empirical features of this sort are not to the point. Certainly I can *show* that a given conclusion is no good because when tested it turns out to be false. But, by that time, the original "inference" has joined the domain of the I.P. On the other hand it is the very essence of the notion of I.I. that when I draw it I am never to be taken as being in the position of having as yet tested it, or of being able to imagine any way in which I could suggest any such tests, apart from those which, forming part of the I.P., led to the formulation of the law itself.

On a certain criterion of meaning, if one cannot show what would make for or against a certain position, that position is senseless. On that criterion, Mill's assertion that I.I. is valid, since the validity involved is not open to the usual verification tests, would have to be accounted literally senseless. However, we need not be over-fastidious with our criterion of meaning. A more serious and interesting consideration is this: Mill's position had been to suggest that there was involved in induction a step or passage which led to the acquisition of new knowledge, new propositions. The Milne-Newton-Schlick doctrine implied a denial of this: thus Schlick had said, a law does not represent so much a proposition as a specification for the formation of propositions. It now appears, however, that if Mill is to maintain that there is involved in the formation of laws an additional question, viz. verifiability in the future, he can do so only by understanding this to mean that the possibility of verification must for ever remain an open question. If so, Mill's position would not differ very much from that of Schlick's.

One is really pulled in opposite directions. We asked: "I have good and sufficient evidence for this law but does it follow that the law is true?" One would not normally want to answer Yes to this question. But can one answer No? Suppose we said, as a very rough approximation, that when we say "the law is true" we understand this by imagining what the facts would have to be like for it to be true if we made all the requisite investigations. And one might say No to our question because after all one's evidence can be "fallible", in Mill's sense. However, this—as we saw when studying Mill's position—is not to the point. We are not interested in the fallibility of the evidence, but in its validity; i.e. we are presumed to have done all that can be expected of us. It might still be objected that we do not know what will happen in the future; but as we saw, this objection also could have no telling force with Mill, for we aren't interested in the question of a changing world (granted even that we could

tell what this means). But in that case, as I showed, we cannot imagine the circumstances under which it would be correct to assert that we had good and sufficient evidence, and to deny that the law was true. So it is a little odd to answer No to our question. And yet, as I said, it would be equally odd to answer Yes to it off-hand.

Now the trouble is that I am not quite sure whether Mill, or anyone who thinks like Mill, is saying Yes or No. The nearest to which I can come is this: Mill seems to be saying: though it doesn't *follow*, nevertheless when you have good and sufficient evidence, it is as a matter of fact permissible to assert that the law is true; and just because it does not *follow*, this is a significant assertion. But I must say that the precise nature of this significance systematically eludes me.

Or perhaps Mill is just saying that inductive inference is a rational and independent form of inference. In the light of Mill's argument this must be a metaphysical position (though corresponding to a P-mood position in identical terms). Mill's position has a meaning only in the sense stated and is not open to any empirical or logical tests. *A fortiori*, it makes no sense to ask for a justification of this position. I think the most pervasive elements in the history of the induction controversy has been the error of assuming that since a position such as Mill's has a meaning therefore it must make sense to ask for a justification. This, if the arguments in this paper have any force, would seem to have been a mistake.

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LOGICIANS AT PLAY; OR SYLL, SIMP AND HILBERT

By A. N. PRIOR

There is a widespread impression that formal logic is in a hopelessly enfeebled condition in the British Isles at present, and flourishes only in America. And there is some excuse for this impression; formal logic, carried on intensively by modern techniques, does not occupy a very large place in the Philosophy curricula of British universities, and Strawson's very slighting treatment of the subject is not untypical of philosophers of the prevailing school (though it is not completely typical either—the "prevailing school" is by no means such a monolithic affair as it is apt to seem from a distance). But there *are* formal logicians in the British Isles today; doing as good work, I suspect, as is being done anywhere.

This has been partly due to the influence of American guest lecturers, notably W. V. Quine and Hao Wang. The latter now has a permanent Oxford appointment; and the "metamathematics" which one associates with such writers as Quine, Gödel and Kleene is also plied in Oxford very vigorously and uncommonly lucidly by M. W. Dick, after an American training. But this American influence has not been by any means the only one. The fact that Łukasiewicz spent his last years in Dublin is having its effect—in Dublin itself (where those who knew him still refer to him simply as "The Professor", much as Aristotle was just "The Philosopher" to the schoolmen); in Belfast; and also in England, where logic is being done in the Łukasiewicz manner by D. P. Henry in Manchester, by Alan Rose in Nottingham, and in Oxford by E. J. Lemmon and by Father Ivo Thomas of Blackfriars, to mention no others. Manchester has also now acquired C. Lejewski, who was just due to have a doctorate conferred in Warsaw when all that was interrupted by war and exile, but who did receive it from Łukasiewicz's hands in the end, after having qualified again in London under Popper. In Oxford, too, there has been the persistent cultivation of logical interests, regardless of the subject's place in the general estimation, by W. C. Kneale and his wife—and Kneale, it should be emphasised, is not just a historian of formal logic, nor even just *the* historian of formal logic (though he is that too, or the two of them jointly are), but has made independent advances in

the subject, as anyone who reads the new *Contemporary British Philosophy* volume can see. There has also been, in Oxford, the interest taken for one reason or another in Frege. In Cambridge there is a flourishing school of mathematical logic in the Mathematics Department (one of its latest products, Dr. T. Smiley, startled P. T. Geach and myself—we had not encountered this particular peril before—by the ease with which he floored both of us in a Joint Session discussion at Aberystwyth). One might mention other factors, and other people; but that will do by way of generalisation.

I was unable to visit Dublin before Lukasiewicz died there (February, 1956), but I was there later in the year, working with C. A. Meredith; and this is a man of whom I want to say more, as his work is not much heard of, and yet it is of quite extraordinary excellence.

To judge from what he and others have so far published, one would say that Meredith's outstanding logical achievement has been the discovery of some remarkably short single axioms for the propositional calculus enriched by a variable δ , such that δp can stand for any function of p that can be constructed in the system.¹ Lukasiewicz used one of them, $C\delta p C\delta N p \delta q$ (for a system with C and N both undefined), as one of the two asserted axioms in a modal logic to which he was attracted in his later years; the other being a specifically modal axiom, either $CL p p$ (with L undefined and M defined as NLN) or $Cp M p$ (with M undefined and L defined as NMN).² This is a curious system, since although Meredith's axiom seems reasonable when there are only two-valued truth-functores to substitute for δ , the substitution δ/M which is possible in a modal system gives $CM p CMN p M q$, "If p is possible, then if not- p is also possible, anything at all is possible". Meredith's axiom is deductively equivalent to an ordinary basis for the propositional calculus without δ , plus the "law of extensionality" $C\bar{E} p q C\delta p \delta q$, "If p and q have the same truth-value, then if any function of p , the same of q ". This is again odd when we let δ be M ; most of us would be inclined to say that p and q might both have the same truth-value, namely falsity, and yet p be possible and q not possible. And with this Meredith would agree; the law of extensionality which he has admitted into his own (as yet unpublished) modal system with δ is the more qualified one $CLE p q C\delta p \delta q$, asserting that if p and q are necessarily equivalent, then any function of the former

¹ See my report "Lukasiewicz's Symbolic Logic" in this *Journal* for May, 1952, pp. 45-6; and my *Formal Logic*, I.iii.4 and I.iv.3.

² These variants are given in the Appendix to Lukasiewicz's "On a Controversial Problem of Aristotle's Syllogistic", *Dominican Studies* VII (1954).

(expressible in the system) holds for the latter also. His system amounts to Lewis's S_5 with this law added, though his actual axiomatic basis is one equivalent to this but shorter. It is like Meredith, too, not just to dismiss Lukasiewicz's system as eccentric, but to see in it a first attempt at what no one had previously tackled, the construction of a modal system with δ occurring essentially in it, and then to try and do the same thing properly.

When meeting him and watching him at work, the impression that he is a first-class tidier-upper is confirmed, but what one notices most of all is his extreme familiarity with the logical behaviour and deductive possibilities of formulæ in which no constant is used but implication. One tends normally to think of the calculus of pure material implication—the one for which Lukasiewicz found the shortest single axiom, $CCCpqrCCr pCsp$ —as a mere fragment of propositional logic, requiring to be supplemented by negation and its laws. But there is another way of looking at this. When one requires the full propositional calculus, one usually wants more as well—the propositional calculus is usually only a basis on which we build something else, such as quantification theory or modal logic or deontic logic; and if within this added discipline we can construct a formally false proposition, we can add the discipline in question to the merely implicative calculus and prove the full propositional calculus as it were in passing, by defining Np as CpO (where O is our defined false proposition) and either laying down or deducing the thesis COq . For example, if the added discipline is quantification theory our O can be 'Everything is true', $\pi p p$; and I have lately struck two other O 's, one of them Meredith's own.

As an optional extension of his modal system, Meredith has worked out the theory of a propositional constant n , which he calls (following Wittgenstein) 'the world', because it means 'Everything that is the case'. n is the conjunction of all true propositions; one of its laws is $CpLCnp$, which holds because if p is true it will be one of the conjuncts of n , and therefore necessarily implied by it by $LCKpqp$. Thus understood, n obviously cannot itself be our "standard false proposition" O ; but Ln can, if we are prepared to assume that there are true contingent propositions (for the conjunction n will then have contingent components, and in consequence *not* be necessary). The other O is an American one, and arises thus: A. R. Anderson of Yale has recently constructed a system of deontic logic³ in which the one

³ Due to appear in his *The Formal Analysis of Normative Concepts*, Technical Report No. 2, U.S. Office of Naval Research Contract No. SAR/Nonr.-609(16), 1956.

basic term added to those of ordinary modal logic is a propositional constant P , meaning some evil thing, not further specified, but such that " q is forbidden" may be defined as " q strictly implies P "; the received principles of deontic logic being then deduced by ordinary modal logic from the assumption that the Bad Thing is avoidable, NLP . Bausch of Chicago has observed that this makes it possible to make LP our standard false proposition for completing the propositional calculus in the usual way from its pure-implicational segment. (This suggestion gives an interesting relation between " q is false", " q is forbidden" and " q is impossible"—the first works out as $CqLP$, the second as $LCqP$ and the third as $LCqLP$). When this was passed on to Meredith, he remarked that now that we have a logic of the World and a logic of the Devil, all that we need is a logic of the Flesh.

This procedure of starting from implication is one which I personally find very attractive; it appeals to whatever there is of traditionalism in me, for implication, of one sort or another, surely is traditionally the logician's first concern, and even if we must sooner or later bring in other matters too (especially negation), I find it pleasant to delay this as long as possible, and to see how much can be done with implication alone. C. S. Peirce used to say that a purely implicational logic is the logic of the age of innocence, and all the rest is imposed on us by the Fall;⁴ and I think he felt, and was right in feeling, that not only in these formalisms but even *philosophically* negation is a parasitic conception. There, incidentally, is another debt to America; if English or at any rate Irish logic is at some points even ahead of American, it is partly because what Peirce began was continued in Warsaw, and brought from there to Dublin.

It is, moreover, astonishing what *can* be done with implication only. For example, we can express in purely implicational terms the difference between what depends intuitively on the law of excluded middle as well as the law of contradiction and what depends on the law of contradiction only. Intuitively the law of excluded middle is presupposed when we use the constructive dilemma $CCNpqCCpqq$ or its condensed form $CCNpqp$; but it is only the law of contradiction that is behind our use of the destructive dilemma $CCpNqCCpqNp$ and its condensed form $CCpNpNp$. Hence of these four dilemmatic formulae from the classical C - N calculus, only the second pair are in the intuitionist calculus of Heyting. But $CCNpqp$ follows by the substitution q/O from the purely implicational formula $CCCpqpp$ (Peirce's

⁴ C. S. Peirce, *Collected Papers*, 3.488-9. Cf. 3.191, 2.356.

own "icon" for the law of excluded middle⁵), and $CCpNpNp$ by the same substitution in $CCpCpqCpq$;⁶ here again the second formula is in intuitionist logic and the first is not. In fact an axiom-set for intuitionist pure-implication can be obtained by adding $CCpCpqCpq$ to $CCpqCCqrCpr$ and $CpCqp$, and for classical by adding $CCCpqpp$ to the same two other axioms. Lukasiewicz condensed the basis for intuitionist implication to $CpCqp$ and $CCpCqrCCpqCpr$; and Meredith pointed out to me that the second of these formulae (which was used by Frege in 1879 in the first complete axiom-set for propositional calculus) is just the implicative form of the full destructive dilemma (r/O turns it into $CCpNpCCpqNp$). The implicative form of the full constructive dilemma is $CCCpqrCCprr$, which was Tarski's distinctive axiom for C -classical before Bernays showed that $CCCpqpp$ would do. Meredith has himself reduced Lukasiewicz's two axioms for intuitionist implication to one; and, with another achievement of the same sort, I was in at the kill. But more of that later.

Returning now to the general scene. There *are* logicians in England and Ireland; but it must be admitted that they are somewhat scattered, and so far as I could gather they had never had any general get-together before July, 1956, when an informal logical "colloquium" was held over a week-end in Oxford. Those present included Faris and Nidditch from Belfast—Faris axiomatised the five Eulerian relations in the *Journal of Symbol Logic*; Thomas has since much simplified that—and Bednarowski, another Euler-expert, from Aberdeen; Lewy and Smiley from Cambridge, and Bennett just appointed there; M. and A. Lazerowitz, on a visit from the States; and others mentioned below. I am sure most of their deliberations will appear in print in one form or another within the next few years, but I offer in what follows what amounts to a review of this group of articles before they are published, in the hope that some of those who are following this subject will find it useful.

The colloquium was opened by Thomas with a paper on syllogistic logic with quantifiers. Lukasiewicz's basis for the Aristotelian syllogistic, it may be recalled, consists of the axioms (subjoined to the propositional calculus) Aaa , Iaa , $CKAcbAacAab$ (Barbara) and $CKAcbIcaIab$ (Datisi), with A and I undefined and E defined as NI and O as NA . Subjoin syllogistic to proposi-

⁵ C. S. Peirce, *Collected Papers*, 3.384.

⁶ Cf. my "Curry's Paradox and 3-valued Logic" in this *Journal*, Dec., 1955.

⁷ And he has a nice model of classical implication within intuitionist—definite Gpq as $CCCqppq$, and one can prove G -detachment, $GpGqp$, $GGpqGGqrGpr$ and $GGGpqqp$ from the intuitionist axioms for C .

tional calculus and quantification theory instead of to the former alone, and certain simplifications immediately become possible. In the first place, Iab can be defined as $\Sigma cKAcaAcb$, and Iaa and Datisi proved from Aaa and Barbara with this definition. Secondly, Aaa and Barbara, with the rules for quantifiers, have the same logical force as the single axiom $EAab\pi cCAcaAcb$. (That this follows from Aaa and Barbara is proved in my *Formal Logic*, II.i.3; the converse can be shown easily enough.) So this suffices, in syllogistic with quantifiers, to prove whatever follows from the Lukasiewicz basis. But once quantifiers are introduced into syllogistic, new truths in the field are formulable for which this basis is demonstrably insufficient. For example, $COab\Sigma cKAcaEcb$. (The truth of this may be informally verified by using Euler's diagrams. Oab is true if and only if either b is wholly included in a but not *vice versa*, in which case that part of a which is outside b is a c such that Aca and Ecb ; or a and b intersect, in which case that part of a which is outside b is again a c meeting the conditions of the formula; or a and b are wholly outside one another, in which case the whole of a meets the conditions for c .) Thomas produced a basis closing such gaps as this, and enabling us to turn directly into deductions, not the Eulerian decision-procedure for syllogistic logic, but Thomas's own decision-procedure as sketched in *Mind*, Oct., 1952, and in *Dominican Studies* V (1952). Free use was made of the form Ta , " a is atomic" or "There is exactly one a ", definable in quantified syllogistic as $\pi bCAbaAab$. If empty terms were admitted as values of the variables, this would only mean "There is at most one a "; but for that matter if empty terms were admitted Iab , defined as $\Sigma cKAcaAcb$, would be a logical law (since $KAcaAcb$ always holds with a null c)—this calculus is just not designed for this interpretation.

A system designed, like quantified syllogistic, to be a theory of terms or common nouns, but with empty terms admitted, was described by Lejewski in the paper which followed, a remarkably lucid account of Lesniewski's ontology. For bringing out the meaning of the wealth of statement forming functors of term-arguments which this discipline contains, Lejewski put up an ingenious extension of Euler's diagrams, with plural terms represented by circles with lines attached, singular terms by dots with lines attached, and empty terms by bare lines; the five Eulerian possibilities being thus considerably enlarged. A number of Lesniewskian propositional forms (e.g. "The a exists", "An a exists", "No a exists", "At most one a exists", "The a is a b ", "The a is the b ", "Some a is a b ", "Every a is a b " with and without implication of the subject's existence) were explained by

indicating which diagrams verified them and which did not (e.g. "The a is a b " is true when a and b are singular and coincident and when a is a singular term within a plural b ; and false in all the other cases involving two terms); specimen equivalences relating these forms (e.g. "An a exists" = "For some b , the b is an a ") were listed; the principles on which Lesniewski constructed his axioms and definitions were explained; and new axioms of Lejewski's own, with different forms taken as undefined, were put up.

Lemmon gave us, in the evening, a paper on the weaker Lewis modal systems, and others allied to these. Lewis's own postulate-sets for his modal systems have a certain untidiness—on the side of definition, their undefined constants are the two truth-functors N and K supplemented by the modal functor M ("Possibly"), strict implication $C|pq$ being defined as $NMKpNq$; but their axioms and rules do not consist analogously of the ordinary truth-functional calculus with a modal supplement, but are all expressed in terms of strict implication, the propositional calculus (in N and K) being deduced as a by-product. Gödel and von Wright have produced 'supplemented propositional calculus' versions of Lewis's two strongest systems S_4 and S_5 , and Feys and von Wright of a system called T or M which stands with S_3 between S_4 and S_2 , but neither contains S_3 nor is contained in it. But no one has published similar versions of systems weaker than S_3 or of S_3 itself; and Lemmon's paper was an attempt to fill this gap. (It emerged from the discussion that Smiley had been occupied with the same problem, reaching substantially the same results.) The Gödel-Feys postulates are: for T , subjoin to the propositional calculus the modal functor L ("Necessarily"), the definition of M as NLN , the rule that if a is a theorem so is La , and the axioms $CLpp$ and $CLCpqCLpLq$; for S_4 , add to T the further axiom $CLpLLp$; for S_5 , add to T the axiom $CNLpLNLp$. Lemmon replaced the three Gödelian axioms for S_4 by the pair $CLpp$ and $CLCpqLCLpLq$; and obtained T by retaining the S_4 rule to infer La as a theorem from a as a theorem, but weakening the second axiom to $CLCpqCLpLq$ (this is just Feys's basis); and S_3 by retaining the S_4 second axiom but weakening the rule to "If a is a propositional-calculus tautology or an axiom, La is a theorem". (It's like "can't curl and can swim— S_3 , that's him".)

Lemmon also described systems, different from any of the Lewis ones, in which the Gödelian rule was still further weakened to "If $Ca\beta$ is a theorem, so is $CLaL\beta$ ". In these, no theorem whatever begins with L , and he suggested that here we might interpret Lp as " p is a natural but not a logical necessity".

Weaken these "E-systems" in turn by replacing $CLpp$ by $CLpMp$ and we obtain "D-systems" in which Lp might be read as "It is obligatory that p ". It was pointed out in discussion that theorems beginning with L would be obtainable in both types of system by adding ΣpLp to the axioms, but Lemmon argued that neither "Something is scientifically necessary" nor "Something is obligatory" ought to be laid down as laws of the *logic* of scientific necessity and of obligation.

Modal logic is, I suspect, something that all logicians play with, whether it is their main business or not. D. P. Henry of Manchester, whom one mostly noticed in free moments discussing computer circuits with Rose of Nottingham, told me just before Lemmon's lecture that he had defined a modal logic within the propositional calculus by defining Mp as Czp , where z is a variable not put to any other purpose. (As the answer to " $p?$ ", "Possibly" means "Yes, if —".) From this information alone it would seem that this system would boil down to Lukasiewicz's, for $CpMp$ follows by substitution (q/z) in $CpCqp$, and $CMpCMNpNq$ by substitution in $CCrpCCrNpCrq$.⁸ On the other hand, I have myself constructed an equivalent of Mp in terms of *strict* implication in Lewis's S_5 . Since any necessary proposition, and only a necessary proposition, is strictly implied by any other necessary propositions, such as $C|pp$, $Lp = C|C|ppp$. Further, in S_5 if a proposition strictly implies its own necessity then its very possibility implies its necessity, so that a proposition strictly implies its own necessity if and only if it is either necessary or impossible, i.e. 'p is either-necessary-or-impossible', call it Rp , $= C|pLp$. And a proposition's being either-necessary-or-impossible strictly implies its truth if and only if it is not impossible, i.e. is possible; so $Mp = C|Rp = C|C|pLpp = C|C|pC|C|pppp$.

Meredith, the morning after Lemmon's paper, described some of his advances in a direction which for most of us was completely new. The following is an ordinary, if difficult, proof by substitution and detachment:

1. $CCCCttpqCrpCCspCuCrp$
2. $CCsCpCpCCCpCpCuCrq$
 $\quad I \ p/s, q/Cpp, r/CCppq, s/CuCrq, u/v$
 $\quad = C_2 \ s/CCts \text{ — } 3$
3. $CCCuCrqsCuCCCpCpqs$.

⁸ Moreover, this two-valued modal logic is not all that different from my own two-valued interpretation of Lukasiewicz's system (*Formal Logic*, pp. 208-9), viz., that his M is a variable such that Mp may be either p itself or the tautology Cpp . Henry's z may be true and may be false; if true, $Mp = C1p = p$; if false, $Mp = COp = 1$.

That's how it's written out in the books and articles, but a man who's doing detachments all the time is liable in private to be content with this (after 1 and 2 are down):

$$1 = C2-3. \quad CCCuCrqsCvCCCp\dot{p}qs.$$

(Given 3, you *can* work out for yourself what substitutions in 1 and 2 will lead to it.) And, Meredith pointed out, still further abridgement is possible. Using *Dab* to mean "the result of detachment with *a*, or some substitution in *a*, for your *Caβ*, and *b*, or some substitution in it, for your *a*", we could write the proof as

$$D12 = 3. \quad CCCuCrqsCvCCCp\dot{p}qs.$$

But this explanation of *D* is not quite accurate. There is no such thing as "*the* result of detachment etc." in this case; for example, 3 with *u* instead of *v* could equally well have been obtained by substitution and detachment from 1 and 2. But 3 with *u* instead of *v* would be a less general result than it could be, for the two *u*'s instead of a *u* and a *v* would impose a restriction on later substitutions. *Dab* is not just any, but the most general possible result of the sort indicated; and 3 is in fact *D12* in this sense. This notation makes possible the condensation of whole sequences of substitutions and detachments into a single proof-formula; e.g. *DaDbc* instructs us to perform a detachment with *a* for our "major", and for our "minor" the result of detachment with *b* as major and *c* as minor; *DDbca*, on the other hand, puts the result of detachment with *b* and *c* into the major position for the final detachment. For example, if 1 is *CCp\dot{q}CCqrCp\dot{r}* and 2 is *CCCp\dot{q}p\dot{p}*, *DD12 = CCpCp\dot{q}Cp\dot{q}* (derivation of intuitionist pure-implicational calculus from classical—see above).

Using the abstract forms *DaDbc* etc., detachment may itself be made an object of systematic investigation; and by substituting for *a*, *b*, *c* etc. constants which stand for particular formulae, we may systematically investigate the deductive possibilities of the formulae in question. I shall not give details at this point; but it was here that some of us encountered for the first time the useful and agreeable Dublin habit, contracted from Lukasiewicz, of referring to various key formulae by proper names; in some cases by the names used in *Principia Mathematica* (e.g. 'Simp' for the Principle of Simplification *CpCq\dot{p}*) and in others by the names of logicians associated with the formulae. Thus *CCCp\dot{q}p\dot{p}* is called "Peirce"; *CCCpqrCCp\dot{r}*, "Tarski"; *CCpCp\dot{q}Cp\dot{q}*, "Hilbert"; *CCpCqrCCp\dot{q}Cp\dot{r}*, "Frege"; *CCNp\dot{p}p*, "Clavius"; *CpCNp\dot{q}*, "Duns Scotus"; and of course there is "Lukasiewicz" himself, *CCCpqrCCr\dot{p}Csp*. They begin to sound a bit like beasts

in a private menagerie, especially when they are spoken of by people who know their ways well, and one sees the point of Hegel's famous comparison of the classification of logical forms with the classification of parrots.

Kneale wound up, that afternoon, with a very straightforward account of the "inference-schemata" of the Stoics. An excited whisper of "Hilbert!" was heard from the Irish contingent when the speaker put up the Stoic proof, from simpler principles, of the schema "From 'If p then if p then q ', and p , infer q ". Geach, in the chair, gave a nice example from Anselm of a complex inference with a thoroughly Stoic flavour: "If God can be conceived to exist but does not exist, then He can be conceived to begin to exist; but God cannot be conceived to begin to exist, therefore it is not the case that He can be conceived to exist but does not exist; but He can be conceived to exist; therefore He does exist." Mediaeval logic is of course Geach's known *expertise*; but the discussion of this paper gave several people whose reputation mainly rests on their systematic work an opportunity to display some unexpected erudition—one was reminded again with something of a shock that some of our best technical logicians, e.g. Lemmon and Lejewski, were classical scholars first.

As is usual with such gatherings, much went on behind the scenes; but of this I can only record something in which I was personally involved. It was provoked in the first place, I think, by Lemmon's paper. There are two ways in which Lewis's modal postulates may be made somewhat tidier. We may, with Gödel, Feys, von Wright, and now Lemmon and Smiley, assimilate the axioms and rules to the definitions by presenting them as a modal superstructure on a propositional-calculus foundation. Or we may do the opposite, and make strict implication a starting-point in the definitions as it is in the axioms and rules. If we do this, the proper first step is to formalise that part of each Lewis system which can be expressed using *nothing but* strict implication (and variables). This was pointed out to me by David Meredith (a cousin of C.A.'s who is in the same line of business) in a punt on the Cherwell after the colloquium was over. Why, he asked, has there been no calculus of pure strict implication, as there are calculi of pure material implication and pure intuitionist implication?

I thought about this problem afterwards, and tried to make a start on it, at least with the strict-implicative fragment of S5 (the earlier-mentioned definition of M was a by-product). Discussions with Lemmon followed, and correspondence with

Ireland, and when I was in Dublin a few weeks later C. A. Meredith was working on it in earnest. Before I left he had proved that three fairly simple axioms, with substitution and detachment, sufficed for pure strict implication in S_5 . After I had left he sent me on a postcard the deduction of these three, with the proofs all packed into the D form, from a single axiom, in fact from the formula labelled 1 at the beginning of my account of Meredith's colloquium paper, which expresses a law when C is strict as well as when it is material (the formula labelled 2, of which the same is true, is not an independent premiss, but is $DDDIIII$). How this postcard covered with what was obviously a message in code was allowed to reach me from a foreign country I do not know, but I shall have to have it framed.

In modal logic, as in assertoric, it has turned out to be remarkable how much you can do with implication alone, of the appropriate sort. For example, you can differentiate purely in terms of strict implication between S_4 and S_5 . If you take the three axioms for 2-valued implication (Syll, Simp and Peirce), replace the material implications by strict ones, and the variables by strict sub-implications, all three of the resulting formulae hold in S_5 ; but in S_4 you can only do this trick with the axioms for *intuitionist* implication (Syll, Simp and Hilbert).⁹ And this accords intuitively with the usual method of differentiating the systems by reference to concatenated modalities. In S_5 all assertions of necessity (and a strict implication is one kind of assertion of necessity) are themselves either necessary or impossible, so that at this level S_5 naturally has a 2-valued look. But in S_4 , where we have $CLpLLp$ but not $CNLpLNLp$, we can only say that assertions of necessity are themselves either necessary or *false*, which gives the system the same rather engaging asymmetry or limp that one observes in intuitionism.

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⁹ That S_5 is to S_4 as classical logic is to intuitionist was not discovered in 1956, though it was then set in a new content. It was noted by S. Halldén in a thesis reviewed in *Journal of Symbolic Logic*, XVI (1951), p. 70.

TOYNBEE AND THE PROBLEM OF HISTORICAL KNOWLEDGE

By C. B. JOYNT

Arnold J. Toynbee is the author of a long list of historical works covering an astonishing range of time and materials. As the editor of the *Survey of International Affairs* he has directed, organized and often personally written many volumes of a penetrating character dealing with the world scene since the war of 1914-18. He is best known, however, for his world-famous series *A Study of History*, wherein he surveys the vast panorama of the past and advances a complex theory which attempts to explain the rise and fall of civilizations. The *Study* has had enormous influence not only in historical circles but among intellectuals in general. It is only right, therefore, that it should be subjected to the most careful and searching scrutiny, for the series raises issues of fundamental importance for the future of historical studies. This paper will put forward a preliminary critique of Toynbee's philosophy of history. It does not contain an explicit criticism of his theory concerning the rise and fall of civilizations although it does have serious implications for the latter.

The size and scope of the *Study* is so vast that it cannot be examined in detail. It is therefore proposed to select for examination a large segment of volume nine entitled "Law and Freedom in History".¹ In this section Toynbee's philosophy of history is fully revealed but since it is clothed in vague and abstruse language it has proved necessary to handle it by examining (1) those concepts and categories which are purely empirical in character and (2) those concepts and categories of a metaphysical nature.

Toynbee cites three major concepts—insurance tables, business cycles, and war-and-peace cycles—as examples of what he calls "laws of nature". The first two concepts have been well established and have proven to be fruitful generalizations. They therefore pass unchallenged here. The third hypothesis, which

¹ Arnold J. Toynbee, *A Study of History*, vol. 9 (London, 1954), 165-405.

originates with Toynbee, proves on examination to be much less satisfactory than the other two. The following table gives his data.²

	Overture.	First Regular Cycle.	Second Regular Cycle.	Third Regular Cycle.	Fourth Regular Cycle.
	A.D. (1494-1568)	A.D. (1568-1672)	A.D. (1672-1792)	A.D. (1792-1914)	A.D. (1914-)
(i) Premonitory Wars (the Prelude) ..	..	..	1667-68	..	1911-12
(ii) The General War ..	1494-1525	1568-1609	1672-1713	1792-1815	1914-18
(iii) The Breathing Space	1525-36	1609-18	1713-33	1815-48	1918-39
(iv) Supplementary Wars (the Epilogue) ..	1536-59	1618-48	1733-63	1848-71	1939-45
(v) The General Peace ..	1559-68	1648-72	1763-92	1871-1914	..

The five periods he has labelled as "Breathing-Spaces" saw some 62 wars waged, including the Peasants' war, the series of Ottoman wars, the British civil war, the war of the Quadruple Alliance, the first opium war, the Manchurian hostilities, the Italo-Ethiopian war, the Spanish civil war and the Sino-Japanese war.³

The five periods Toynbee calls 'supplementary wars' provide excellent examples of the curious way in which facts are unconsciously chosen to fit a predetermined thesis. The gigantic and destructive Thirty Years War, which included almost all the great powers of Europe, and the most awful war ever fought in history, the war of 1939-45, become "supplementary", though the latter was fought on every continent and ocean, bringing down in ruins several of the great powers and laying waste vast sections of Europe and Asia.

His five periods of 'General Peace' afford no greater satisfaction, for they encompass periods of history in which there occurred approximately fifty-two wars, including such vital episodes as the Great Northern War, the Ottoman War, the Anglo-Dutch Naval War, the American Revolution, the French Revolution, the Franco-Prussian War, the Russo-Japanese War, the Sino-Japanese War and the first and second Balkan Wars.

Toynbee's penchant for theological explanation as the ultimate kind of explanation of historical events is clearly demonstrated by his analysis of such an obviously empirical concept as the Balance of Power. After showing that there exists a general movement from a system involving a plurality of States to the

² Toynbee, *op. cit.*, 255.

³ Quincy Wright, *A Study of War*, vol. 1 (Chicago, 1943), 641-647.

creation of a universal empire he then falls back upon the statement that "at bottom" all these developments have been and are due to the "sinfulness . . . of human nature".

Quincy Wright in his monumental *Study of War* has collected a vast amount of data on this problem which indicates that war is not a function of human nature as such but of social customs and institutions.⁴ Wright has also shown that the collapse of balances of power in the past can be adequately explained by such factors as the rise of a conquering genius, the invention of new military weapons of great offensive value, the polarization of power within a particular system, the impact of powerful states on the periphery, the elimination of small states, and the propaganda of new religions.⁵ Why then is it necessary for Toynbee to invoke such a slippery and unsatisfactory concept as 'human nature' to explain the breakdown of balance of power systems? Surely a gentle application of Occam's razor, which forbids the multiplication of hypotheses beyond those necessary to explain phenomena, is called for.

In his efforts to answer the question "are human affairs governed by law?" Toynbee plunges so deeply into metaphysical categories that it is difficult to know precisely what it is he is saying. Taking a strongly dualistic view of the mind-body relation, he first attempts to define the terms "human affairs" and "laws" in what he calls "the spiritual sense".⁶ They do not mean "medicine, but the humanities; not the organic chemistry, biology, and physiology of the human body, but the affairs of human beings in that spiritual aspect of Humanity in which man is a person with a consciousness and a will moving on the face of the waters of a subconscious psychic abyss".

For Toynbee the field of human affairs apparently consists of four provinces, "the Soul's diverse relations with God, with its own self, with a relatively small circle of other human beings with whom it is in direct personal communion and with a relatively large circle of people with whom it is in indirect impersonal contact through the mechanism of institutions".⁷

Toynbee argues that human affairs are governed by two kinds of law—"the laws of nature" and the "laws of God". These laws can apparently be conceived by the human mind. Indeed "the laws of nature" are apparently accessible to a mysterious, undefined entity called "the Collective Human Intellect".⁸ These

⁴ Wright, *op. cit.*, vol. 2, 704, 1198, 1226.

⁵ Wright, *op. cit.*, vol. 1, 760-766.

⁶ Toynbee, *op. cit.*, 169.

⁷ *Ibid.*, 168-169.

⁸ *Ibid.*, 169.

⁹ *Ibid.*, 17.

"laws of nature" are inexorable and unchangeable. Relief from them can only be sought in "the laws of God". For Toynbee, although the term "laws of God" is neither explained nor defined, any incompatibility between these two concepts of law is dismissed as "merely a mirage in the shadow-world of abstract logic". In real life he holds that they are involved "in a divinely inspired interplay" in which, at four different levels of Reality, the cyclic movements of the law of nature are transcended. The day and year cycles are transcended by the individual's life-time, the biological cycle by civilization, and the latter in its turn is transcended by "the cumulative spiritual progress of Religion".¹⁰

The failure of other historians to identify these "laws" in history is apparently due to a variety of factors. Early writers allegedly failed because they adopted a naïve and distorted version of the Christian *Weltanschauung*;¹¹ others, like H. A. L. Fisher and R. G. Collingwood, allegedly fell into erroneous ways by their sheer obsession with historical technique and the delusion that the historical process was so complex as to prohibit the formation of general laws of history.¹²

At any rate the operation of these "laws of nature" produces a process of disintegration in civilizations. To the inquiry as to whether these laws are controllable in the sense that collapse might be averted he is forced to reply that the empirical evidence offers little hope.¹³ He holds out some hope that states might avoid such a collapse by voluntarily agreeing to a union, but he warns us that "constitutional arrangements can never serve as a substitute for the spiritual redemption of souls".¹⁴

In this apparently hopeless dilemma Toynbee invokes the concept of "freedom". "Challenges from God may evoke creative responses in human souls that are genuinely free human acts . . ." ¹⁵ These "challenges from God" and their human responses are apparently the counterpart, on a spiritual plane, of his famous challenge-and-response thesis which he offered in previous volumes to explain man's ability to surmount difficulties and problems at the economic, political and technological level of human experience.¹⁶ It is quite significant that whereas Toynbee is able to cite many examples of challenge-and-response at the

¹⁰ *Ibid.*, 174.

¹¹ *Ibid.*, 175-178.

¹² *Ibid.*, 206; cf. for example R. G. Collingwood, *The Idea of History* (Oxford, 1946), and H. A. L. Fisher, *A History of Europe*, 3 vols. (London, 1935).

¹³ Toynbee, *op. cit.*, 344.

¹⁴ *Ibid.*, 347.

¹⁵ *Ibid.*, 382.

¹⁶ *Ibid.*, 391.

latter level¹⁷ he is not able to bring forward one specific example of the *spiritual* type of challenge-and-response which he postulates. He can only say that of twenty-two previous civilizations only the western is still alive and he clearly implies that somehow (unexplained) the West has risen to the Divine Challenges—challenges which he does not specify and enumerate.

Those historians like H. A. L. Fisher who have tended to assert that history was knowledge of individual fact and not the knowledge of general laws are bitterly attacked by Toynbee.¹⁸ He asserts that the human mind is "so constituted as to be intrinsically incapable of ever *thinking* about anything at all except in terms of uniformities, recurrences, rhythms, plots and patterns of other kinds, while, conversely, none of these patterns is conceivable as being anything but an arrangement of facts".¹⁹

This view of how we attain historical knowledge is much like the view of J. Huizinga when he argues that:²⁰

"The idea of history only emerges with the search for certain connexions, the essence of which is determined by the value which we attach to them History is always an imposition of form upon the past and cannot claim to be more. It is always the comprehension and interpretation of a meaning which we look for in the past."

Both these arguments show great imprecision in that they fail to distinguish clearly between the methodological and ontological status of uniformities. If these rather loose formulations are meant to show that we read laws into history that are not there but are purely mental constructs, then it can be said with confidence that such a view of history is not only gratuitous but also inconsistent.

One of the objections to the view that historical laws are objective regularities rather than mental constructs is the erroneous belief that historical determinism is equivalent to fatalism. This would mean that the advocacy of political and social action would be inconsistent with the belief in the objectivity of historical laws. For example, it is often argued that

¹⁷ *Ibid.*, 382-391. The challenges he cites were the threat of Islam in the fifteenth century and the rapid rise of population following the Industrial Revolution. These were met by the creation of ships capable of circling the globe, thus enabling the Western powers to break out of Europe, by the rise of agricultural and industrial production and the expansion of ship-building, which enabled these extra mouths to be fed through production for export.

¹⁸ *Ibid.*, 200-201. Their philosophy of history is not only attacked but they are accused by him of agnosticism, atheism and rather inconsistently of antinomianism, i.e., the belief that faith alone is necessary to salvation.

¹⁹ *Ibid.*, 201.

²⁰ J. Huizinga, "A Definition of the Concept of History" in Raymond Klibansky and H. J. Paton (eds.), *Philosophy and History* (Oxford, 1936), 5 ff.

Marx was precluded by his own theory of historical determinism from advocating changes in the structure of society on the alleged grounds that his theory presumably predicted the occurrence of these changes independently of such action. The fallacy in any argument of this type against historical determinism (the Marxian form of historical determinism being only one) is that "the system concerning which a historical determinist makes a prediction *ab extra* . . . can easily be one in which he is a causal element along with others who desire to achieve certain goals and who urge still others to share these ambitions". Such predictions are "predicated on the existence of persons like himself as *participants* within the system and not on the fatalistic view that the desired outcome is independent of the existence of such persons".²¹

How can the historian 'impose' upon history a 'form' for which he still looks? It might be more sensible to argue that historians bring particular hypotheses to a study of history and then arbitrarily select facts to uphold these hypotheses. This would be to "impose a form" on the past but it is certainly not to look for *meaning* in the past. Rather, it is to bring meaning *to* the past.

Again, it is surely obvious that while history is sometimes an imposition of form upon the past in the sense of Huizinga, more often history, in the shape of the facts, can itself defeat and conquer a particular form (hypothesis) brought to it. For example, Citizen A may come to the study of Hitler's regime in the belief that all men are rational. It is not beyond the bounds of possibility that even a summary appraisal of Herr Hitler's political and racial theories would convince him that his assumption was quite incorrect. It seems likely that Citizen A would be forced by the facts to revise his proposition at least to the extent of saying that "All men but Hitler are rational". The relation of history and the human mind would therefore appear to be dialectical in the sense that they constantly act and react upon one another, producing fresh insights into the relations between facts.

What really disturbs Toynbee is the problem of how to make sense of history—in short, how to find some meaning in the historical process. The haunting fear that the problem may be insoluble appears again and again and no more clearly than when he bitterly attacks those eighteenth-century historians and

²¹ Adolf Grünbaum, "Time and Entropy", *American Scientist*, vol. 43 (October, 1955), 569-570; and see his "Historical Determinism, Social Activism and Predictions in the Social Sciences", *British Journal for the Philosophy of Science*, vol. 7 (Nov., 1956), 236-240.

their spiritual descendants who declared (according to Toynbee) that no sense was to be made of human affairs.²²

Here Toynbee seems to be confusing two quite different uses of the word 'sense'. Most eighteenth-century historians surely would not deny, as H. A. L. Fisher did not deny, that one could discern small patterns in history that made 'sense' in the following way—namely, that factors leading up to particular events could be listed, discussed, weighed and refined until a modest empirical statement could be issued relating them to these events. What they did deny was that any single, overall meaning or pattern could be derived from the historical phenomena which they were attempting to explain. Toynbee, on the other hand, is trying to find the nature of ultimate reality in the patterns of history, and he claims to have done all this through "an empirical survey of the data".²³

This is rather an astounding claim and should be subjected to close scrutiny. In what sense are categories such as the "Laws of God" and the "Collective Human Intellect", to name but two, empirical? If one assumes that the method of science is to explain phenomena by using hypotheses which find relations among these phenomena, and that these hypotheses must be "capable of at least partial or indirect confirmation or disconfirmation by means of some data of observation",²⁴ it must be concluded that his categories are not empirical at all. Historically speaking, categories such as the above have no meaning. Since questions arising out of these categories are so constructed that they prevent responsible answers, it is correct to say that the categories succeed only in raising a host of false problems.

In short, Toynbee is guilty of using pseudo-explanations. These differ from scientific explanations in that the latter are capable of being objectively checked. These checks include (1) an empirical test of the sentences which describe the initial and boundary conditions for the occurrence of the event, (2) "an empirical test of the universal hypotheses on which the explanation rests" and, finally, (3) an investigation as to whether the sentence asserting the event to be described can be logically deduced from (1) and (2).²⁵

Once include the kind of category Toynbee includes and history as such simply vanishes. Once scientific methodology is

²² Toynbee, *op. cit.*, 197.

²³ *Ibid.*, 218.

²⁴ Herbert Feigl, "Aims of Education for our Age of Science", *Fifty-Fourth Yearbook of the National Society for the Study of Education, Part I* (Chicago, 1955), 315.

²⁵ Carl G. Hempel, "The Function of General Laws in History", *Journal of Philosophy*, vol. 39 (January, 1942), 36-38.

abandoned and writers fail to state what kind of evidence is relevant in the testing of their hypotheses and what findings tend to confirm them we are launched on a vast sea of verbosity without a compass to steer by. When history goes the fairy tale takes its place.

There can be no compromise on such a vital point. Either we have history or we have myth. No half-way house is possible.

The conclusion forced upon us by this analysis is that Toynbee shows a meaning and a purpose in history by imposing on the miscellaneous facts of history a pattern laid up in heaven, a pattern supposed to be empirically derived and demonstrable. Far from giving us a pattern so derived and so demonstrable, he blurs all historical categories of meaning under the lash of his religiosity. Categories which are the result of empirical generalizations, such as the balance of power and business cycles, are treated *on the same level of meaning* and as if they were derived *in exactly the same way* as "the laws of God".

If this kind of practice is to be pursued then historians might as well shut up shop and all take holy orders. For Toynbee's method ends in the destruction of history. He abolishes history in the sense that he abolishes the empirical categories which make it meaningful. Not only does Toynbee's cavalier application of spiritual concepts tend to break down painfully erected categories of meaning but his importation of theological language tends to destroy the quality and meanings of words as well. For example, he talks of "the epiphany of the Modern Western Balance of Power". The word "epiphany" means, according to Webster, "a manifestation, especially of divinity". Whatever else can be said of the term balance of power surely we can secure general agreement that it does not partake of the divine. This is only one example among many, for religious terms abound in the work, and many of them are grotesquely applied.

In short, Toynbee has attempted to blend two kinds of knowledge: religious and historical. The epistemology of the latter is empirical and it cannot be successfully joined to the former. What Toynbee has written is really much closer to theology than history. The danger is that the reader, blinded by the vast apparatus of learning and swept on breathlessly from historical to theological categories and back again, will be tempted to believe that he is witnessing a perfectly sound example of historical explanation. What he is really witnessing is a subtle blend of some sound history and a great deal of theology. Indeed, Professor Toynbee has come much closer to *summa theologica* than to *summa historica*.

In some ways the Toynbee view of the meaning of history is odd even by theological standards. The traditional Christian view is that the ultimate meaning of history is eschatological, i.e. it lies beyond itself. Toynbee claims to have used empirical methods, and produced not only "laws of nature" but "laws of God" as well. In other words, the ultimate meaning of history is to be found in history. In this conception the Deity becomes almost entirely immanent and loses *all* transcendent qualities.

The student of history has a right to ask the question "in what sense, if at all, is Toynbee's kind of history valid?". The answer would appear to be that every man is his own historian and that every man has a perfect right to construct heuristic pictures of the past or to construct poetic versions of the historical process as a psychic aid to himself or others. It is in this sense that Toynbee's history is "valid". It is to be seriously doubted that much of the study goes beyond this, for in vast sections the only description which could be applied accurately is "mythology".

Whatever else history is, it is not myth. The essence of mythology is an effort to state by means of metaphors and analogies experiences which transcend the senses. There can be a history of myths but there can be no history which consists of myths as part of its structure of meaning. At such a point history becomes metaphysics, religion or poetry.

Such history is private, purely subjective history. It can be shared only by those who accept the metaphysical presuppositions involved. In Toynbee's case these are so strange that vast sections of his history are solipsistic in content.

If one is charitable and defines history for every man as "any account of past events", one must not make the mistake of assuming that all such accounts are equally valid and meaningful. They are not all equally valid because they do not all explicate their basic assumptions with equal care, and they do not take into account with equal scrupulousness facts which might embarrass their hypotheses. They are not all equally meaningful because they vary greatly in the range and extent of the empirical hypotheses by means of which they systematize seemingly disparate and unconnected events. In short, there is enormous variation in the historian's grasp of the ways and workings of man and the institutions which he creates, and which in turn influence their creator.

These distinctions point to the conclusion that, though "everyman" may be and often is his own historian, all history is not and cannot be of that kind. If we begin by asking how other kinds of history differ from that of "everyman" the general answer is that such accounts are not just any accounts of past

events but are rather "any coherent pattern of significant events".²⁶ But what is meant by "coherent" and "significant"? My Aunt Sarah played golf last Saturday and shot a round two under women's par. This activity could be described both as "coherent" and "significant", yet it does not warrant inclusion in a history of Western civilization. Unless such an activity could be shown, for example, to form part of a larger social pattern (how a middle-class woman's leisure activities demonstrate her system of values, say) then such an activity has no significance for the historian. The burden of demonstrating the significance of the pattern he has selected is on the historian. The next question is "How *can* he, how *must* he demonstrate its significance?". He can rely on some private vision or intuition which conveys meaning and significance to him. That is his privilege as a human being. But history viewed as a series of empirical hypotheses can claim no such privilege. They are not private visions but public assertions. When Toynbee says his history is scientific he is saying, among other things, that it is publicly testable. The necessity for this is clear: unless such a rule were enforced there would be no accepted methods of deciding between opposing points of view. This is the chief charge which can be made against Toynbee's kind of history. He often writes on levels of generality and in a context such that there are no rules by which his assertion can be checked and a dispute with him settled.

The plain truth is that, though there can be a history of metaphysics, metaphysics is not history. To confuse the two levels of generality is to pile confusion on confusion and the end result is to reduce a discipline to mere airy speculation and often to meaningless assertion.

Toynbee's history is a kind of twentieth century searching for the City of God. Like all such philosophies of history it lays claim to more than finite perspective. For Toynbee really pretends to give an estimate of the total significance of the historical process, thus supplying indirect evidence of the dangers of the *hubris* he castigates so strongly. For to give an estimate of the total significance of the historical process is to pretend to the attributes of a transcendent mind. Pride could hardly carry anyone further than that. One can but conclude that this curious marriage of metaphysics and history has not succeeded and should end in a divorce.

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²⁶ Dorothy M. Emmett, *The Nature of Metaphysical Thinking* (London, 1949), 161-162.

DISCUSSION

R. F. HOLLAND ON 'RELIGIOUS DISCOURSE AND THEOLOGICAL DISCOURSE'

By J. M. CAMERON

Much in Mr. Holland's hard but fascinating paper seems to me to be right.

(a) God as a hypothesis designed to account for the world's being *thus* and not *thus*; God as *an* existent or an existent *of a certain sort*. Concepts of this type are useless for the reasons he gives and for other reasons. But—to distinguish my agreement with his main point from my disagreement with much he attaches to it—isn't it true that the reasons for thinking concepts of this type useless are to be found in some of the classic theologians (e.g. in the scholastic dictum that God is *extra omne genus*)? And in rejecting these concepts for reasons given, isn't Holland himself doing theology and theology of a scientific kind (after all, when theology was called a *scientia* this involved no claim that it was 'scientific')?

(b) Holland seems to me clearly right about Professor Wisdom's *Gods*. It is quite extraordinary that Wisdom should in this paper have concerned himself with Wordsworthian pantheism and with spirits of the grove and stream and not with the central religious concepts of his own culture. That this paper should have been considered an important contribution to the philosophy of religion emphasizes in a singular way the state of mind of many contemporary philosophers.

(c) Holland has shown the immense difficulties raised by the notion of peculiar and special religious experiences, whether these are held to be self-authenticating or not. I am only a little unhappy about what he has to say about St. Paul's experience on the road to Damascus. He argues that *if* Paul's experience is to be counted as an experience of God, then this will be on account of what happened afterwards; and that 'if after his experience he had continued to live as he had lived before, then the experience would have been shown not to have been an experience of God'. This seems to me to mean that if there are

good grounds for asserting that S is P, then the absence of these grounds entitles us to assert that S is *not* P. I agree that Paul's subsequent life provides us with good reasons for asserting that the Damascus road experience was of a certain sort, and that, had Paul continued to be a persecutor, we should no longer have the same good reasons for so characterizing his experience. That Paul's continuing to be a persecutor would *show* the experience not to have been an experience of God seems to me false. That my conduct from T^a to T^b is of a certain sort does not entail that at T^a my experience was or was not of a certain sort.

My first general difficulty about Holland's paper is concerned with the way in which he sets the scene. This he does by describing the discourse of religious people and distinguishing it from the discourse of theologians, pagans and perhaps some others. (He seems to stipulate that 'religion' shall be used in such a sense that it does not apply to polytheistic religions. Why? More should be said about this.) He asserts that talking *about* God 'is not obviously essential to' the way of life of religious people. He can only say this because he has neglected one category of discourse, namely, preaching. This is most obviously central in Protestant Christianity, but it is in fact also important in Catholicism, in Orthodoxy, in Judaism and in Islam, all of which would (I take it) come within the definition of religion stipulated by Holland. We may take as the archetype of preaching St. Paul's discourse to the Athenians in which, telling them about the unknown God they ignorantly worship, he certainly talks *about* God. It would seem hard and a trifle absurd to describe this and other preaching which attempts a similar task as theological and non-religious discourse. Even Holland wishes to connect faith with certain logical and/or ontological categories. He wishes to say — this is surely the point of his stipulative definition of religion — that believers in many gods have made a mistake. Christian faith has a content which can be analysed (one of the jobs of the theologian) and analysis shows that the content includes statements about God's character, activity and relation to the world. I cannot think that Holland means by what he has to say about theology that the kind of analysis of the content of faith which Athanasius or Augustine or Newman or Barth or de Lubac gives us is nonsensical theology, though I am not clear how he would characterize such work. (Incidentally, in taking as his theologians Papini and C. S. Lewis, isn't he making the same kind of mistake as Wisdom in *Gods*? If we may so define religion as to exclude superstition, we may so define a theologian as to exclude these gifted writers. I think he in fact mistakes the genre to which Lewis's writings belong. They are lay sermons

of an overwhelmingly cosy sort. I share what I guess to be Holland's feelings about them; but I think our objections to them are aesthetic.)

In what Holland says about the bogus character of some theology I take him to be saying this. If to theologize is to look for a *logos* of God, this is a plain mistake. Haven't the great theologians known this quite well? God *is* the Logos and the Logos is God. To ask for the logos of the Logos is to repeat in more sophisticated terms the child's question: Who made God? Aquinas, for example, distinguishes between our knowing *quia deus est*—this is to know there is a Logos of existents—and the question *quid deus sit*, which, he thinks, we don't and can't know. If this is Holland's point it seems to me true and important, though I am not clear that it entails the impropriety of all discourse about God, in *all* senses of such discourse and in *all* senses of (logical) impropriety.

My greatest difficulty is to find Holland's meaning when he argues that such statements about God—he acknowledges they belong to religious as distinct from theological discourse in the pejorative sense of the latter—as 'God is my creator' would be better described as statements 'not so much about God as about the believer'. He offers as a logical model of such a statement: 'I owe my life to my parents'. 'God is my creator' is a statement about God in the same way, and only in the same way, as 'I owe my life to my parents' is a statement about my parents. I don't quarrel with this. He then goes on to argue that if anyone hearing 'God is my creator' asks for a *description* of God, 'this request signifies a misunderstanding'. I can fully understand the expression 'I owe my life to my parents' without being given a description of the parents in question and the same may be true of 'God is my creator'. But an inquirer might well think that he understands 'I owe my life to my parents' only because he knows a parent when he comes across one; and he may profess to be unable to understand the expression 'God is my creator' because he isn't able to give the word 'God' a meaning in the way he is able to give 'parent' a meaning. This *would* be a misunderstanding, but not at all because 'God is my creator' can more properly be said to be a statement about me than a statement about God. That we can construe 'God is my creator' in the way Holland thinks we can is not a reason of any kind for saying that we can't discuss *quid deus sit*, that is, ask for a description. And it is only *philosophically* (and, in one sense of the word, *theologically*) improper to ask for a description. On the level of religious discourse we do give descriptions to inquirers and we don't (ought not to) tell them their question is an improper one.

On the level of 'religious discourse' the request for a description *isn't* improper. If there is a sense in which it is improper, this is a sense which occurs only in the kind of 'theological discourse' Holland seems to think nonsensical or beside the point.

My question to Holland can be put in this way. Throughout he is using criteria to distinguish between what it makes sense to say about God and what it doesn't make sense to say about God; to distinguish between 'God' and 'gods'; to distinguish between 'religion' and 'superstition'. He does not give all his criteria. One criterion used is how things look for one who uses religious discourse sincerely and is committed to the way of life of religion. Thus, he doesn't feel called upon to explain or to justify his attitude to 'the greedy nephew's conception of God'—he speaks of its *obvious* degradation'. Again, in discussing Wisdom's *Gods* he argues that one who treats the existence of God as an experimental issue is *'ipso facto* concerning himself with something other than God and religion'. This is to ask us to take religious discourse and the religious way of life as data. But so for some are the pagan and superstitious way of life and discourse. Does Holland wish to say that our job is simply to exhibit the distinctions between the two sets of data, expose those category mistakes of theologians and anti-theologians which have infected the discourse of simple religious people and simple atheists, and then take to our prayers, or not, as the case may be? I suspect that he is using other criteria and that these are of a philosophico-theological kind. It strikes me as being of some significance that after his attack on the employment by theologians of such terms as *infinity* and *finitude*, *necessity* and *contingency* and so on, he finds towards the end of his paper that when he has to speak about faith he can only do so by having recourse to this suspect terminology. He tells us that 'it is by its infinitude that the faith of religion distinguishes itself from pagan faiths, it is as an object worthy of an infinite faith that the God of religion is distinguished from pagan gods'. His point may be that what is vicious is the application of these notions to the elucidation of the nature of God—to the search for a logos of the Logos. But what does he think the fathers of Chalcedon should have done in 451? They were faced with those who said that Christ was altogether a creature; that he was not a man; that the Divine and the human co-existed in him but were not united. They replied in terms two of which were drawn from the philosophical word-book of the period. Christ was held to be, as touching his Godhead, *homoousios* with the Father; the Godhead and the manhood were held to be united in one *hypostasis*; and in consequence Mary could be truly said to be

the *Theotokos*. This does not sound like religious discourse in Holland's sense: it is theological discourse. But such theological discourse has manifestly *determined the character of Christian religious discourse* (thus indirectly providing Holland with his criteria for distinguishing religious from other discourse) and distinguishes it from Socinian or Muslim or Jewish religious discourse.

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THE STATUS OF THEORETICAL ENTITIES

By J. P. MCKINNEY

In summing up his interesting study of the problems raised by the "entities" of modern theoretical physics, Professor Smart says (this *Journal*, May, 1956, p. 10): "We cannot see electrons and there are theoretical reasons why we could never in principle see them. And that is about all there is to it."

This suggests that, when it has been shown, as I think it can be shown beyond question, that the part that the electron plays in the structure of modern theoretical physics precludes our speaking of it in the terms we apply to the objects of our macroscopic experience—that once this has been shown, that is the end of the matter. I would say that, on the contrary, it is only the beginning of the matter; that, far from solving our problem, we have only succeeded in stating it, and it is only from here on that discussion becomes fruitful. For the question now arises, what are the theoretical reasons for the peculiar status of these subnuclear entities, and how have such reasons been arrived at?

As Smart himself has indicated, when we ask a question such as "Are electrons real?" we are not, in fact, asking about the nature of "electrons". More fundamentally, we are asking a question about the nature of "reality". This gives rise to a peculiar situation which is pregnant with the sort of confusions of thought on which controversies thrive. For before we can answer the question, Is an electron, as a basic element in the structure of the world of our common experience, real in the same sense that the things constituting the world of our common experience are real? we must first arrive at a generally accepted definition of "reality" as applied to the objects of our common

experience. And *that*, it may be remarked, is the concentrated essence of the whole traditional problem of philosophy.

But this points to another and highly significant impediment that blocks the way to a solution of the problem of electrons. This may be stated baldly as follows: *The difficulty we have in defining the reality of the electron arises from the fact that the electron is itself part of our definition of reality.*

I am quite aware that the point I am trying to make here is extremely elusive. But I am convinced that it is as significant as it is elusive—that, in fact, our present difficulties have arisen precisely because the significance of what we are doing eludes us. It is not easy, in the first place, to realise that, when we ask about electrons, we are really asking about reality. But it requires a further and even more difficult effort of intellectual detachment to realise that the structure of ideas in which “electron” occurs—the “theoretical reasons” referred to by Smart—is the answer, or part of the answer, to the question about reality.

Perhaps I can indicate what I am trying to get at by glancing, briefly, at the world-picture of contemporary physics, as contrasted with the “classical” world-picture of half a century ago. The chief point of difference, and what gives to current views their highly revolutionary aspect, is that whereas the classical physicist regarded the subject of his investigation as being a world which was the independently-existing *cause* of experience, the modern physicist sees the world which is the subject of his investigation, as being (in some as yet not clearly defined sense) a *construction from experience*—a view which, it may be noted, has been confirmed by the independent investigations of the logical analysts.

Now the significance of this change of viewpoint does not lie, as is usually suggested, in a simple change from one way of looking at the world to a different way of looking at it. It does not mean that the classical physicist had one way of looking at the world he was investigating, whilst the modern physicist has another way of looking at it, as for instance men once thought that the world was flat, and then, by the accumulation of evidence, came to realise that it was round; or as they once believed in the Ptolemaic cosmology, and then came to accept the Copernican. Those changes of viewpoint concerned only the *content* of men’s thought-processes. But the present shift from the “classical” to the “modern” world-view concerns not only the content—*what* men think—but the nature of the thought-processes themselves—*how* they think. Once the world is seen as, not the independently

existing *cause of* experience, but a *construction from* experience, the very nature of our inquiry has undergone a transformation. And this commits us, not simply to a new conception of what we know, but—what is much more revolutionary—to a new conception of knowledge itself. For it is now seen that we are inquiring into a structure that is a relationship between experiences, and not into a structure that is a relationship between entities existing independently of experience.

This is the straightforward implication of the view that the “world” is an experiential construction. But it is an implication which many thinkers, including such initiators of the modern revolution of thought as Einstein and Planck, have found it hard to accept. It seems to fly in the face of both the past development and the present success of physical inquiry. If all we are talking about is experience, and if experience means—as it seems that, in the final analysis, it must be accepted as meaning—individual experience, then the Reality which seems to be the very basis of physical inquiry collapses into what Kant called a “mere rhapsody of perceptions”, or, less poetically, a sort of mudbank of solipsism where knowledge founders and is engulfed.

For these very persuasive reasons, one school of thought feels impelled to reject the more disturbing implications of the modern revolution of thought, and to seek for some theoretical reconstruction that will allow the retention of “objective reality” as the subject of physical inquiry. Another school, impressed by the logic of the modern revolution of thought, wants to accept the full implications of the present standpoint—a world-picture based on discontinuity, indeterminacy and probability—trusting to the powers of the modern intellect to then devise some theoretical expedient that will reinstate Reality as a “limiting case” or other such theoretical correlate of the present standpoint.

Between these two schools of thought, and between the various shades of opinion that have developed within each school, as each proposed solution has given rise to fresh difficulties, there seems to be no possibility of a reconciliation. One type of thinker feels secure only if he can retain “objective reality” in its pristine integrity—though how this is to be achieved is largely a matter of personal taste. The other feels that, armed with the modern techniques of thought, he can make do with a reality that shows a certain infusion of experience—though the extent of infusion that is permissible is not easy to decide. But there is one point on which both schools of thought and all their various shades of opinion are agreed, and it is a very curious point.

All parties to the present conflict of standpoints are agreed that there is only one way of solving our present problems, and that is by way of a *theoretical* solution. And the peculiar feature of this is that, *faced with problems arising out of the development of empirical thought, we abandon the empiricist attitude in favour of theoretical speculation.*

The mediæval scholastics asked, How *could* motion occur? The empiricist, Galileo for example, asked, How *does* motion occur? The first took refuge in speculation, the latter turned to experiment. When Galileo set up an inclined plane and rolled a series of balls along it, he exemplified the attitude of mind which has led directly to the triumphs of empirical science—and finally to the problems of modern thought. At which point, abandoning the empirical attitude, we ask, not, How have these problems arisen? but, How can we set up a theory that will dispose of them? Perhaps we have not yet become enmeshed in the toils of a modern scholasticism, but in our retreat from the empirical attitude we have at least taken the first steps in that fatal direction.

But—the question now arises—how are we to find a way of escape from this dilemma? Not—I would say the answer obviously is—from any standpoint within the present imbroglio. That way we could go on arguing these problems interminably, each choosing as his *starting-point* the *conclusion* that he finds temperamentally congenial. Then, how? I have already suggested the answer to this question: by asking ourselves, not How can we find a theoretical way of escape from these problems? but How, as a matter of empirical fact, have these problems arisen?

Here is a certain state of affairs, a complex condition of thought. Offhand, we might say that, being a problem of thought, it can only be solved by a deliberate exercise of the full powers of rational thought, i.e. theoretically. But the difficulty is that it is a problem involving the foundations of rational thought itself. And indeed, looked at in this way, we might even suspect that the present state of thought presents itself to us as a problem because it involves a shift in what we might call thought's centre-of-gravity, and until we have defined, and adapted ourselves to, that shift, we cannot get anything into a true intellectual focus. In that case, the only procedure open to us is to ask—not, How can we solve our problems theoretically?—but, What is the aetiology of the present crisis in thought, what are the historical and logical steps by which the present state of thought has come about?

This brings us back to our original question: What is the status of electrons? I suggested at the outset that the electron

is part of the answer which modern physics gives to the question as to the nature of reality. And what is the answer that modern physics gives?—if, indeed, we shouldn't rather regard what we call "modern" physics as being this answer. Anyway, the answer physics gives is, as we have already noted, that reality is a construction from the data of experience.

But, it will be objected, this simply lands us back in the toils of the problem we set out to escape from: the dilemma that the objectively real, and so knowledge itself, is reduced to a rhapsody of subjective experiential data that precludes knowledge and anything we would call the objectively real. But, the objector might justly insist, we *have* knowledge, in the sense of a communicable system of ideas, and that this is knowledge of something other than our own experience is proved by the actual physical power that has accrued to us by the progressive development of this knowledge, and in particular the very developments that constitute the modern revolution of thought.

But all this does not constitute a problem. It is simply a concise statement of the significant situation to which the development of thought has brought us today. Let us look directly at the facts, and not start tinkering with them to make them suit our preconceived ideas.

Fact No. 1: We *have* knowledge, in the sense of a common body of communicable ideas, valid for all experients.

Fact No. 2: This knowledge has increased with the historical development of thought and in particular with the present revolutionary developments of thought.

Fact No. 3: This historical development has consisted in tracing the structure of knowledge, the structure of the common body of ideas that constitute our world-picture, back to its basic elements and constitutive principles.

Fact No. 4: The outcome of this investigation has been to show that this common world-picture, the conception of a determinate objective world of things in time and space, is a construction from the data of experience: that it has been built up, by a process of "statistical correlation", out of the discontinuities and indeterminacies of subjective individual experience; or simply that *common objective* knowledge has its basis in *subjective individual* experience; or, conversely, that private subjective experience has been raised, through the ages, to the level of common communicable knowledge.

Those are the facts of our present situation. They are all, I would say, straightforward enough. And they clearly do *not* mean that there is *nothing but* experience, that our common

experience, our established body of knowledge, has no objective correlate; that, in other words, there is no such thing as knowledge because there is nothing to know, or alternatively, though we may assume that there is something, we cannot in any meaningful sense know it. On the contrary, the final and most significant fact of the development of thought is that the *further we have penetrated into the structure of experience, the greater has our knowledge become*: this is the fact which, until we take an objective view of our present situation, presents itself in the paradoxical form that every advance of knowledge seems to involve a breakdown of knowledge. And that this increased knowledge is a knowledge of something other than experience is evidenced by the fact that *every step back into the structure of experience has been accompanied by a step forward in our insight into and power over the conditions that govern our experience, or what we call the World or Reality*.

Looked at in this way, Reality or the World emerges not as the independently-existing starting-point or cause of knowledge—which “as such” we must declare, with Kant, to be unknowable—but as the end-point or guarantee of knowledge. Emerging as the end-product of inquiry, it both guarantees reason and is guaranteed by reason. Which seems to be that marriage of Faith and Reason which men throughout the centuries have been trying, hopefully but never quite successfully, to achieve. Here we seem to have achieved it despite ourselves; or should we perhaps say, it seems to have achieved itself through us?

Incidentally, this raises an interesting point in regard to the generally accepted view that, as Smart expresses it, the physicist's world-picture is truer, i.e. is a more accurate account of reality, than our common-sense picture, because it is less anthropomorphic, i.e. contains fewer elements of experience; or, as it has been expressed elsewhere,¹ that the development of physics has consisted in a progressive elimination of the elements of experience from our structure of knowledge. There are seen to be peculiar difficulties about this view, once we accept the verdict of the present development of thought, that “reality” and “knowledge” mean “common experience”, and that the development of physics has consisted in progressively tracing this body of common experience back to the elements of individual experience from which it has taken its rise.

My purpose has simply been, by treating as the beginning of the matter the point which Smart marks as the end of the

¹ L. von Bertalanffy, “The Relativity of Concepts”, *Philosophy of Science*, Oct., 1955.

matter, to draw up a trial balance, a very bald and superficial one, of the modern revolution of thought. I have also tried to indicate that, whereas a theoretical approach to the present "problems" of thought can only lead us into the quagmire of a modern scholasticism, an empirical approach makes clear that what, from a theoretical standpoint, appear to be problems, and insoluble problems at that, are in fact solutions of problems. *They are solutions of the problems that have been the stock-in-trade of traditional philosophy since its inception in ancient Greece.*

Does this sound too startling and too bold a claim? Personally, I can see no escape from it—unless we are to prefer old and comfortable confusions of thought to new and challenging insights. But today no such choice seems to be open to us: even our familiar and comfortable confusions are becoming increasingly uncomfortable. Our situation seems to have become so critical that only Truth can save us. And for fallible man, Truth is simply facts and their immediate implications.

It will perhaps be pointed out that I still have not really answered the question I started from: What is the status of electrons? My reason for not attempting to answer it is that I don't know the answer. I only know—and I find it a highly suggestive and puzzling fact—that by the manipulation of the formulae in which we express the discontinuous and indeterminate elements of experience that lie at the basis of our common world-picture, we are able to bring about fundamental and, because not fully understood, terrifying changes in the structure of matter. As I say, I do not know what this means. I only know that it is just the sort of fact that, because its implications elude and defeat us, we are tempted to theorise out of the way. But that, of course, is to cut ourselves off from ever understanding it.

North Tamborine, Queensland.

REVIEWS

PLATO'S PHILOSOPHY OF MATHEMATICS. By Anders Wedberg. Stockholm, Almqvist & Wiksell, 1955. 154 p. Swedish Crowns 19.

The title of Professor Wedberg's book is misleading, since he deliberately confines his discussion to those theories of Plato's "which locate the objects of mathematics within a presupposed division of the universe", and since he is almost exclusively concerned with confronting the view which Aristotle explicitly attributes to Plato with the Platonic position on this question as it emerges, or as it has been taken to emerge, from the dialogues. Plato says that there are only forms and particulars and that the objects studied by pure mathematics are certain of the forms (the Ideal Numbers and the Geometrical Ideas). Aristotle, on the other hand, attributes to Plato the view that there are forms and particulars *and* certain other entities intermediate between these which he calls *Mathematicals*, and that pure mathematics studies the *Mathematicals* as well as certain forms. By pure mathematics Wedberg understands throughout elementary plane geometry as in Euclid and elementary theory of the natural numbers, which were the most developed branches of mathematics in the fourth century and the ones with which Plato was usually concerned from the point of view of philosophy. Unfortunately Wedberg discusses geometry and arithmetic separately with no indication of how Plato would have taken them to be connected.

Wedberg shows how the evidence is heavily weighted against Aristotle's interpretation; nevertheless the thesis of his book is that it is (p. 12) "clearly proved by Plato's own words . . . that he [Plato] *assumes* the existence of a kind of numbers which, as he describes them, do not agree with his definition of an Idea, but which, as a matter of fact, answer to Aristotle's definition of *Mathematical Numbers*". (Reviewer's italics.)

Whatever the wisdom might be of treating (in practice) the dialogues from the *Laches* to the *Laws* as if they presented one and the same doctrine of forms, and of making part of Plato's philosophical position what can be shown to be *implied* by what he actually held, Wedberg undoubtedly succeeds in showing from the texts that, when Plato actually *does* a piece of mathematics

(as against philosophizing or enthusing about it) however elementary and if only for the purpose of illustrating a philosophical point, he invariably assumes entities which, if they are not Aristotle's *Mathematicals*, are very close to them, and are certainly nothing like Platonic forms or particulars as Plato understood them.

Wedberg treats the question of the "ontological status of the objects of mathematics" as a special case of the "problem of universals". He therefore discusses the theory of forms (in chap. III) before he takes up more specifically mathematical questions, and while he claims no originality for what he says in that chapter, he succeeds in bringing out clearly and succinctly the conflicting strains in the theory of forms and what he calls its "fundamental antinomy". This is the joint assertion of 'The form of *x*-ness is not one of the *x*'s' and 'The form of *x*-ness is *x*' (e.g. Beauty itself couldn't be just another beautiful object and Beauty itself must be beautiful, if anything is). Wedberg succeeds here, where some well-known names in Platonic scholarship have failed. Wedberg does not go on to give reasons for the occurrence of these antinomies within the theory, nor does he point out in any detail the various roles the doctrine plays in Plato's thought, but perhaps this may be due to his attempt to present the theory (and he does this with more success than one would expect) in a quasi-deductive form.

This book is limited in scope but interesting not only to the student of Plato, but also to the mathematician, if he can be persuaded to re-read his subject from the genetic point of view.

The book is attractively bound and the occasional slight departure from English idiom does not seriously detract from its value.

E. ROXON.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

BENNEY, MARK, and GRAY, A. P., and PEAR, R. H. How people vote; a study of electoral behaviour in Greenwich. (International Library of Sociology and Social Reconstruction.) London, Routledge and Kegan Paul, 1956. xii, 227 p. 25s. (U.K.).

BERKELEY, GEORGE. Works, edited by A. A. Luce and T. E. Jessop; volume 8, Letters. London, Nelson, 1956. viii, 312 p. 30s. (U.K.).

BRILL, JOHN. The chance character of human existence. New York, Philosophical Library, 1956. 150 p. \$3.75.

"If the Darwinian theory is to be completely applied it must not only show the evolution of physical man but likewise the evolution of mental man and show how this transition from physical to mental has been accomplished." This the author attempts to do.

BURNHAM, JOHN C. Lester Frank Ward in American thought. (Annals of American Sociology.) Washington, Public Affairs Press, 1956. 31 p. \$1.

FIELD, G. C. Political Theory. London, Methuen, 1956. xvii, 297 p. 18s. (U.K.).

Based on the late Professor Field's lectures to students. Deals with such topics as sovereignty, democracy, liberty, etc.

FLEW, ANTONY, ed. Essays in conceptual analysis, by H. Brothman, E. Daitz, Antony Flew, Peter Herbst, John Hospers, D. F. Pears, J. J. C. Smart, P. F. Strawson, Stephen Toulmin, J. O. Urmson and G. J. Warnock. London, Macmillan, 1956. xi, 265 p. 18s. (U.K.).

Another Flew anthology, this time restricted to "the field covered by Oxford 'Logic' examination papers: morals, politics, aesthetics, the philosophy of science and religion have thus been excluded".

HAAS, WILLIAM S. The destiny of the mind: East and West. London, Faber and Faber, 1956. 327 p. 36s. (U.K.).

Argues that there are fundamental and far-reaching differences between the basic presuppositions of Eastern and Western thought. This thesis is developed by means of a detailed comparison of Eastern and Western philosophy, religion, politics and art.

HOURLANI, GEORGE F. Ethical value. London, Allen & Unwin, 1956. 233 p. 18s. (U.K.).

Arrives at a naturalist and utilitarian theory of ethics from a consideration of usage.

INTERNATIONAL UNION FOR THE PHILOSOPHY OF SCIENCE. Proceedings of the second international congress, Zurich, 1954. Neuchâtel, Switzerland, Éditions du Griffon, 1955. 5v. Swiss fr. 38.

Volume 1, Plenary sessions; 2, Physics, Mathematics; 3, Theory of Knowledge, Linguistics; 4, Philosophy and Science, History of Philosophy; 5, Sociology, Psychology. Most papers are in French, English, or German.

KLAPP, ORRIN E. Ritual and cult; a sociological interpretation. (Annals of American Sociology.) Washington, Public Affairs Press, 1956. vi, 40 p. \$1.

LEWIS, H. D., ed. Contemporary British philosophy: personal statements; third series. (Muirhead Library of Philosophy.) London, Allen & Unwin, 1956. xv, 501 p. 35s. (U.K.).

As in the preceding volumes, prominent philosophers are invited to outline their general philosophical position. Some of them have preferred to illustrate it by discussing a particular problem. Authors are: Aaron, Acton, Ayer, Barnes, Campbell, Copleston, Ewing, Findlay, Hampshire, Hodges, Kneale, Lewis, Mabbott, Mackinnon, Paton, Popper, Price, L. J. Russell, Ryle and Waismann.

MAYER, FREDERICK, and BROWER, FRANK E. Education for maturity. Washington, Public Affairs Press, 1956. vi, 155 p. \$3.25.

"Throughout the book comes the call for an education which will cease being a 'ritual' or a 'mere training' and become instead 'a living faith and a way of life which leads to a brave new world of happiness and peace'." Introduction.

MAYER, FREDERICK, and BROWER, FRANK E. Patterns of a new philosophy. Washington, Public Affairs Press, 1955. vi, 112 p. \$3.25.

"Like Santayana we believe that philosophy is a form of poetry and that the vision of the thinker is more important than his technical proficiency. Unlike Santayana we are optimistic regarding the possibilities of Western culture, especially Western democracy . . . We believe that the philosopher is not only the guardian of reason, but also the guardian of morality." Preface.

MELLO, A. DA SILVA. Man: his life, his education, his happiness. New York, Philosophical Library, 1956. 729 p. \$6.

Translated from the Portuguese. The author is a Brazilian doctor, who gives his opinions on sex, marriage, child development, socialism, religion, etc., etc.

MILLER, OSCAR W. The Kantian thing-in-itself, or The creative mind. New York, Philosophical Library, 1956. xix, 142 p. \$3.75.

The author, who is a psychologist, argues that values are man-made and that concepts depend on interests, and goes on to draw the conclusion that "all of Reality is Mind-Reality. The whole of the Natural World is what it is because of the presence and the activity within it of a cosmic Mind."

NATANSON, MAURICE. The social dynamics of George H. Mead. Washington, Public Affairs Press, 1956. viii, 102 p. \$2.50.

"It is my belief that Mead's work has suffered from well-intentioned but nevertheless mistaken interpretations . . . A close reading of Mead's works reveals a wealth of insights that by far transcends even the broadest conception of behaviorism." Chapter I.

PHILP, H. L. Freud and religious belief. London, Rockliff, 1956. xi, 140 p. 18s. (U.K.).

A defence of religion against Freud. The author is a social psychologist who does not dispute the soundness or the importance of Freudian theory in general.

SARTRE, JEAN-PAUL. Being and nothingness; an essay on phenomenological ontology, translated by H. E. Barnes. New York, Philosophical Library, 1956. lxix, 638 p. \$10.

The introduction is a useful general account of Sartre's philosophy, with special reference to *Being and Nothingness*. The translation is reasonably intelligent, although there are some errors of omission and commission. The book production is defective, e.g. there is no index of subjects.

SEARLES, HERBERT L. Logic and scientific methods; an introductory course. 2nd ed. New York, Ronald Press, 1956. 360 p. \$4.25.

Readable introduction to logic, with lively examples but some serious errors.

SMITH, CHARLES. Sensism; the philosophy of the West. New York, Truth Seeker Co., 1956. 2v., lvi + xviii + 1612 p. \$10.

The author, a lawyer with some training in philosophy, sets out to construct a philosophy in the grand manner, beginning with epistemology and proceeding to morals, religion and politics (including an "Anti-communist Manifesto"). Advocates empiricism, agnosticism and *laissez-faire*, all with variations of his own.

TARSKI, ALFRED. Logic, semantics, metamathematics; papers from 1923 to 1938, translated by J. H. Woodger. Oxford, Clarendon Press, 1956. xiv, 471 p. 86s. 3d. (Australian).

Papers originally published in French, German, or Polish, now translated for the first time; revised by the author.

WOODGER, J. H. Physics, psychology and medicine; a methodological essay. Cambridge University Press, 1956. x, 146 p. 8s. 6d. (U.K.).

The question of how a doctor should be trained leads on to a discussion of scientific method in general and the status of psychology in particular.

NOTES AND NEWS

The Executive Committee of the Sydney Branch of the Australasian Association of Psychology and Philosophy, feeling that it is important that a decision involving the tenure of university chairs should be thoroughly tested in the courts, is raising funds to assist Professor Sydney S. Orr of Tasmania to meet the legal expenses of appealing to the High Court of Australia against the Tasmanian judgment.

Contributions may be sent to the Secretary, Sydney Branch, A.A.P.P., c.o. Philosophy Dept., University of Sydney.

AUSTRALASIAN ASSOCIATION OF PSYCHOLOGY AND PHILOSOPHY, ANNUAL CONGRESS, 1956

The Congress and Annual General Meeting for 1956 were held in Canberra from August 18th to August 22nd. The programme was as follows:

Saturday, August 18th:

8 p.m.—Presidential Address: Mr. Q. B. Gibson, "Social Forces".

Sunday, August 19th:

10 a.m.—Professor J. Anderson, "Philosophical Relations".

8 p.m.—Professor J. J. C. Smart, "The Unity of Science".

Monday, August 20th:

10 a.m.—Mr. C. Hamblin, "Questions".

2 p.m.—Discussion.

8 p.m.—Professor G. Ryle, "Some Myths about Deduction and Induction".

Tuesday, August 21st:

10 a.m.—Mr. P. Gibbons, "Imperatives".

2 p.m.—Professor A. C. Fox, "Reason as Practical".

8 p.m.—Conference Dinner.

Wednesday, August 22nd:

10 a.m.—Professor D. R. Grey, "Truth, Objectivity and History".

2 p.m.—Dr. K. Baier, "The Verification of Value Judgments".

7.30 p.m.—Annual General Meeting.

8 p.m.—Discussion.

